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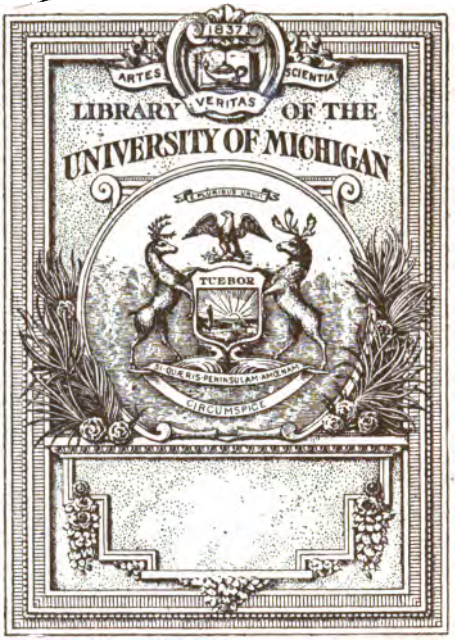

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RETROSPECT

OF

PHILOSOPHICAL, MECHANICAL, *CHEMICAL, AND AGRICULTURAL* DISCOVERIES:

BEING

An Abridgment

OF THE

PERIODICAL AND OTHER PUBLICATIONS,

ENGLISH AND FOREIGN,

RELATIVE TO

Arts, Chemistry, Manufactures, Agriculture, and Natural Philosophy;

ACCOMPANIED, OCCASIONALLY, WITH

REMARKS ON THE MERITS OR DEFECTS OF THE RESPECTIVE PAPERS;

AND, IN SOME CASES, SHEWING TO WHAT OTHER USEFUL PURPOSES INVENTIONS MAY BE DIRECTED, AND DISCOVERIES EXTENDED, BEYOND THE ORIGINAL VIEWS OF THEIR AUTHORS.

VOL. V.

LONDON:

PRINTED FOR THE PROPRIETORS,

AND SOLD BY W. H. WYATT, AT THE REPERTORY OF ARTS AND PATENT-OFFICE, NO. 9, PICKET-STREET, TEMPLE-BAR.

1810.

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RETROSPECT

OF

PHILOSOPHICAL, MECHANICAL,

CHEMICAL AND AGRICULTURAL

DISCOVERIES.

No. XVIII.] *January, February, March.* [1809.

NATURAL PHILOSOPHY, ARTS, AND MANUFACTURES.

A Letter on Comets, addressed to Mr. Bode, Astronomer Royal at Berlin. By Mr. DE LUC.—*Phil. Journ. No.* 98.

THE learned author of this letter, after some remarks relative to the occasion and object of it, commences with what he conceives the whole of our knowledge of nature obviously inculcates, respecting such bodies as are susceptible of becoming *luminous:* viz. "that the light, which then escapes from them, had entered into their *composition* as an ingredient, and is evolved by chemical *decomposition*."

In the application of this principle to the heavenly bodies, he conceives that what Dr. Herschell has advanced relative to the light of the sun not coming from his solid substance, but from some fluid by which he is surrounded, may be extended by analogy to the fixed stars, and that this light is of a similar nature with the luminous appearances observed in our atmosphere. These phenomena, both celestial and terrestrial, he regards as *luciferous* vapours of extremely different magnitudes, which are raised into the respective atmospheres in a compound state, and there, by undergoing a decomposition, emit the light which formed one of their constituent parts. And the changeable spots that are visible on the surface of the sun are considered as proofs that the light of the sun does not issue from his solid body, but from a kind of fluid atmosphere with which he is surrounded; these spots being considered as portions of the opake body of the sun, which are "seen through transparent parts of his *luminous* envelope." This fluid, however, is not regarded as the atmosphere

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of the sun, but merely mixed with it, and not throughout its whole extent, as the spaces through which these spots appear are *transparent*, or such as from which no light emanates. And this hypothesis is greatly confirmed by the consideration that the apparent diameter of the sun's disk, or more properly of his lucid covering, is not subject to any change that we can perceive; whence it is inferred, that this luminous covering maintains a constant uniform thickness, which is not the property of an atmosphere, taken in its general acceptation, or that of being composed of expansible fluids, varying in density with the distance from the body which they surround, and extending through imperceptible gradations to an indefinite distance from their base. Thus, if the luminous fluid with which the sun is enveloped, always preserve such a uniform thickness, and have its outlines so well defined as to be taken for the sun itself, it must consist of a kind of vapour, constantly emanating from the body of the sun, and ascending to the same height in his atmosphere, where it undergoes decomposition, and the light is suffered to escape, which renders it visible to us. The transparent places, through which the opake body of the sun is seen, are regarded as parts of the atmosphere into which these luciferous vapours either do not rise, or are not decomposed; and are analogous to the openings of the clouds in our atmosphere, through which "the blue colour of the transparent air" is seen.

This theory, under certain modifications, the author deems applicable to other celestial bodies, with which our earth "maintains its analogy by the phosphoric phenomena of its atmosphere:" and from this consideration originated the present conjectures respecting comets. He lays it down as a principle, that the light which emanates from phosphoric bodies can arise only from some chemical decomposition: but with respect to the nature of this chemical process, we are far from being able to speak with certainty. The rays of the sun, however, are here regarded as the chemical cause which regulates either the production or the decomposition of these vapours, or both, and gives rise to these luminous effects. Though an approximation to the sun may cause a more rapid decomposition of these vapours, it cannot always produce the same effect, either on different comets or on the same comet at different times, unless the production of these vapours proceed with the same degree of intensity: but which we have no reason for supposing to be the case. If these vapours rise to a considerable height above a comet before they are decomposed, and have not, like those of the sun, any fixed limit of ascent, when the comet approaches its perihelion, and they become luminous, it may appear like a nebulosity only, without our being able to ascertain any nucleus: and when we can perceive a determinate disk, it is probably owing to the regularity

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in the extent of these lucid vapours, without the body of the comet being in the least degree visible.

If such be the nature of the light emitted by comets, and the decomposition which produces it be accelerated in their approach towards the sun, and retarded when they recede from him, the proportion between this acceleration and their proximity may be subject to variation. If then their apparent magnitudes be one of the data in the calculation of their orbits, may not this lead to error? As the light of comets appears to be of a phosphoric nature, which unquestionably depends upon chemical causes, and these seem to be subject to greater variation in comets than in the sun, the difference in the aspect of a comet in its reappearance may be such as to prevent it from being known. So that there is no necessity to suppose a perturbation of its orbit to account for this circumstance.

The tails of comets have always been observed to be on that side of them which is farthest from the sun, and to have their convexities turned towards the side to which they are moving; and the former of these circumstances is here accounted for, by supposing these tails to consist of a fluid similar to the electric fluid, proceeding from the comet, and rendered luminous by a decomposition in its course. The cause of the curvature of the tails is supposed to arise from the particles of the fluid, of which they consist, partaking of the same projectile motion with the comet at the moment they are emitted from it; and having attained a considerable distance from the body of the comet, and continuing to move with the same velocity, but in a larger orbit, their angular motion will be diminished, and the apparent curvature follow as a natural consequence. The length of the tail will be greater as the production of this fluid is more abundant, and its decomposition more slowly effected; "both which circumstances may vary, not only in different comets, but in the same comet at its different returns." And when all these circumstances are combined, is it not possible that a comet that is expected, from calculation, to make its appearance, may not be recognised though it be actually the same?

Observations.—Of all the celestial bodies, comets have given rise to the greatest number of speculations and conjectures; and their appearance has always been a source of curiosity to the learned, and of terror to the ignorant. And notwithstanding all that has been done and written on the subject, from the time of Aristotle and Pythagoras to the present hour, we are forced to confess that our knowledge respecting these wandering bodies is still very imperfect. It is, therefore, a department of science which demands all the vigilance and skill of modern astronomers; and the celebrated Lalande says, in his History of Astronomy, for 1801, "the knowledge of comets is alone wanting to complete

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the science of astronomy." In this state, then, every thing which has a tendency to increase our limited knowledge on this subject, by dispelling the darkness and uncertainty in which it is enveloped, demands not only a candid reception, but also merits particular attention. The present paper bears strong manifestations of the ingenuity of its author, yet we fully agree with him that, "in conjectures like the present, the analogies on which they are founded can furnish only germs of a theory; time alone can shew, by repeated observations, whether they be capable of becoming fruitful; but at least they afford matter for reflection and inquiry." This writer, however, does not seem sensible that the magnitude of a comet is not one of the elements of its orbit, and therefore, that no error can arise in the calculation from this source. Whether or not his mode of accounting for a comet not being known on its return, from its difference of appearance, be sufficient, may in many cases admit of doubt; for provided the comet can be seen at all, and the direction of its motion, its peribelion distance, &c. agree, or very nearly so, with those in its former visit, it would be natural to conclude that it was the same, though concomitant circumstances might have caused some difference in its appearance. The hypothesis advanced by the celebrated M. Lambert, in his Cosmogony, and Mr. Gole, in his Theory of Comets, to account for the same circumstance, is probably not without some foundation. They suppose that the orbit of a comet is not an ellipse, but that when it passes its perihelion, it has acquired so great a velocity, that its centripetal force is overcome by its centrifugal, and that consequently the comet continues to fly off in a parabola or hyperbola, and proceed till it come within the attraction of some fixed star; that this attraction may give it a new direction, and increase its velocity till it come to an apsis below that star, when it may again fly off either in a parabola or hyperbola, and proceed till it fall within the attraction of another star; and thus visit many different systems. Hence it is not difficult to conceive comets which, being attached to no particular system, are common to many, and which, by visiting each in succession, make, if we may so express ourselves, the tour of the universe. The ingenious conjecture of Baron Zach seems to embrace both this opinion and that urged by the author of this letter, to account for the irregular appearances of comets. He says, "Why may not comets appear sometimes luminous, at others dark? The comet of 1770, therefore, might exist sometimes in an opake, and sometimes in a phosphorescent state; and hence, perhaps, and from the perturbative power of the larger and more dense bodies, the unfrequency of their return may be explained. They come back, and we do not see them; they are present, and we do not perceive them."

Admitting these hypotheses, it will follow that the same

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comet may sometimes move in one conic section, at others, in another; and that those which move in ellipses, are not always visible, even in their perihelia.

There is, perhaps, no class of bodies in the whole material universe, respecting which the reins of judgment and philosophic investigation have been so much abandoned to the power of imagination, as in what relates to comets; and the result has been, what it must always be in similar circumstances, a multiplicity of wild, and in many instances, absurd conjectures. One of these is the supposition that the satellites of our system were originally comets, and that they were caused to revolve in their present orbits by falling within the attractive sphere of their primary planets. The supporters of this hypothesis, however, do not seem to be aware of the obvious contradictions and inconsistencies it necessarily involves, by militating against the universal law of gravity, which is so admirably adapted to maintain the order, harmony, and beauty of the universe; and which, from its simplicity, is so indelibly stamped with the characteristics of sovereign wisdom.

If the world be regarded as bearing the impressions, partaking of the character, and answering the benevolent intent of its great Author (and the contrary cannot be admitted), we are induced to conclude that it ought to be as populous as possible, or to contain the greatest number of moving and habitable bodies, of which it is capable, without confusion and disorder. But with this inference, the great eccentricity of many of the cometary orbits has been thought to be irreconcilable, with respect to both the number of moving bodies in the universe, and their habitability, in consequence of the insupportable extremes of heat and cold to which they were supposed to be subject in their perihelia and aphelia. With regard to the first of these, M. Lambert observes, that "the most perfect plan of our system will be that into which enters the greatest number of orbits, all separated from one another, and which in no one point intersect each other. If then we should be able to prove that the orbits of the comets correspond to this end better than those of the planets, the reason of their superiority in point of number must be seen and admitted by minds of the most ordinary capacity." He then discusses this question, whether the greatest number of elliptical or circular orbits can be most conveniently introduced into the solar system? and, upon the principles of the Newtonian philosophy, determines in favour of the former; shewing that the eccentric orbits of the comets, instead of being irreconcilable with the wisdom which is otherwise manifest in the works of creation, are additional proofs that the highest possible perfection obtains in the universe at large. There is reason likewise to believe that the number of comets may greatly exceed what is generally conceived; and the learned author above referred to, after consi-

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dering the subject, asserts, that " a very moderate estimate will give motion in our solar system to at least five hundered millions of comets." If future observations should prove that the nature of comets is what the ingenious author of this letter supposes it to be, and the circumstance is not improbable, the objection to their being habitable on account of their extreme variation of temperature, will lose its force, and each must be admitted to be susceptible of affording life, sustenance, and enjoyment to myriads of rational and intelligent creatures.

From a serious reflection upon the preceding observations, will it not appear that the opinion of those who maintain that all those vast globes which roll with us round the sun, and with us enjoy the benefit of his light and heat, are desolate and destitute of inhabitants, is unsupported by reason, and unworthy to be ranked among the deliberate sentiments of rational and thinking beings? While the contrary supposition exalts the wisdom and benevolence of creative design, far beyond our utmost conceptions, and includes all created nature in that harmonic order which characterizes those parts that fall more immediately under our contemplation. This induced the celebrated author, already referred to, to exclaim, " What a delightful, what an enchanting spectacle is this immense machine, which goes on and maintains its infinitely diversified motions by the most simple of all laws, by the sole principle of gravitation! This is the master-piece of creative Intelligence; an eternal object of admiration to men and angels! "

On barometrical Measurements.—Phil. Mag. No. 130.

The chief part of this communication consists of a translation of M. Ramond's second method of ascertaining the height of objects by means of the barometer, published in the *Mémoires de l'Institut,* vol. vi. and a reduction of his measurements to the measures of this country. In this mode of calculation, the effects of gravity in different parallels of latitude are taken into the account; and the results of the calculation are given in fathoms. In the reduction of the French measures into English, the translator has made use of the following ratios; between the metre and English inches, 1 : 39.371, and for the thermometer 100 : 180.

Two examples of the method of calculation are given. In the former of these, the altitude of Pic du Midi, one of the summits of the Pyrenees, is found to be 1429 fathoms above Tarbes, a town at its foot. In the latter, the height of Mount Salève, in Switzerland, is ascertained to be 472.49 fathoms. In order to accommodate the calculation to the latitude, the coefficient of 45

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degrees is assumed at 1839.3 metres or 10057.6 fathoms; and a table is subjoined, in which the logarithms of these coefficients are given for other degrees of latitude.

Observations.—*Pascal* and *Descartes*, we believe, were the first who proposed the barometer for the purpose of measuring accessible heights and depths; and succeeding philosophers have bestowed considerable attention on ascertaining the proportion between the fall of the mercury in the barometer and the height to which it was carried; and various rules have been given by writers on this subject for computing the height ascended upon this principle. But as we cannot here enter into either a historical detail of the attempts that have been made, or a critical examination of the rules that have been offered towards the attainment of this object, we shall assist the reader in comparing the principal of these rules for himself, by references to the places where they may be found. *M. Daubuisson's* corrections on account of aqueous vapour, with his resulting theorem, are published in the Journal des Mines, No. 113, and noticed in our vol. ii. page 406. The formulæ of *De Luc*, *Bouguer*, and *Trembley*, may be seen in the 3d section of Prony's Architecture Hydraulique; *Laplace's* rule in his Mécanique Céleste, and a modification of it by *Ramond*, in the first volume of Biot's Traité elémentaire d'Astronomie physique. The rules of *Dr. Robison*, *M. De Luc*, *Sir George Shuckburgh*, and *Dr. Hutton*, adapted to English measures, are given in Gregory's Mechanics, book v. chap. 2.

The various circumstances which affect the density of the atmosphere, and are likely to be a source of error in barometrical measurements, have been subjected to investigation by *Professor Playfair*, and an elaborate paper on the subject has been published by him in the Transactions of the Royal Society of Edinburgh, vol. i. And some important correction to the French estimates of altitudes by the barometer, &c. on account of the assumption of 28 French or 29.9 English inches, instead of 30.08 English inches, for the mean height of the barometer at the level of the sea, are pointed out by Mr. *Kirwan*, in his tract "On the Variation of the Atmosphere."

In the present paper we have neither a general theorem, nor any directions for performing the calculation, but are left to infer them from particular examples, to which we must refer such of our readers as are desirous of becoming acquainted with the particulars of the method. This mode of M. Ramond's *appears* to have the advantage over most of the others in point of accuracy, owing to the circumstance of the coefficients varying with the latitude; and this translation and reduction of the numbers from French to English measures will prove acceptable to those geologists who make use of instruments graduated according to the English mode: but here again we cannot speak in that decided

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manner respecting the coefficients which we could wish, as we are *not* made acquainted with the law of their calculation.

The rule investigated by Dr. HUTTON, under the article PNEUMATICS, in his Mathematical and Philosophical Dictionary, and referred to above, is very commodious in practice, and capable of affording a considerable degree of accuracy in the result. It is this: viz. 10000 × log. of $\frac{M}{m}$ is the altitude in fathoms, in the mean temperature of 31°; and for every degree of the thermometer above that, the result must be increased by so many times its 435th part, and diminished when below it: in which theorem M denotes the length of the column of mercury in the barometer tube at the bottom, and m that at the top of the hill, or other eminence; which lengths may be expressed in any one and the same sort of measure, whether feet, or inches, or tenths, &c., and either English or French, or any other nation; but the result is always in fathoms of six English feet each.

Effects of Gravity on the Balance of a Watch compared with those on the Pendulum of a Clock. In a Letter from a Correspondent.—Phil. Journ. No. 97.

THIS writer observes, that the time of the pendulum's descent is in the inverse subduplicate ratio of the force of gravity; but the time of vibration of the pendulum spring, in a watch, is in the direct subduplicate ratio of the weight of the balance. Hence the diminution of gravity, in approaching the equator, must accelerate the motion of the watch, in the same ratio as it retards that of the clock.

"Suppose the diminution of gravity at the equator $\frac{1}{229} = 0{\cdot}004367$. Let the weight of a balance vibrating mean time at the pole be denominated 1. Being removed to the equator, its weight will be $1 - 0{\cdot}004367 = 0{\cdot}995633$, and the time of one vibration at the pole, to the time of one vibration at the equator, will be as $\sqrt{1}$ to $\sqrt{0{\cdot}995633}$, or as 1 to 0·997814; consequently, the number of vibrations in one day at the equator will be to those at the pole as $\frac{1}{0{\cdot}997814} = 1{\cdot}002191$; which reduced to time gives 24ʰ 3′ 9·3″. That is, the watch will go too fast 3′ 9·3″. If the diminution be taken at $\frac{1}{190}$, agreeing with a printed table of the variation of the pendulum to every fifth degree of latitude, then by the same process, the error of the watch will be 3′ 48·3″ daily."

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The magnitude of this error is sufficient to render it easily perceptible in proceeding from a considerable latitude to the line; and if it be found to be just, it will be of a contrary nature to that of the pendulum: the experiments with which, however, are far from agreeing with each other. Comparative observations with the balance and pendulum may be made within about two degrees of latitude of each other; as this distance, according to the table, produces an error in the vibration of the pendulum of 7½ seconds per day; and if the variation of gravity can be ascertained by the balance with equal accuracy, as it is the inertia arising from its weight that regulates the vibrations of the spring, the error will be of a contrary nature, and hence the difference will be about 15 seconds in the space of 24 hours: a quantity very discernible.

With respect to the relation between the weight of the balance and the strength of its spring, this author remarks that, if the balance of a watch be taken off and replaced by another of exactly the same dimensions but double its weight, the inertia will be doubled, and it will require the addition of another spring of equal strength with the former to produce the same velocity. If this last balance be expanded, without losing any part of its weight, until its diameter of percussion become equal to twice its original extent, the inertia will be quadrupled, and it will require eight springs, of equal strength with the first, to cause it to vibrate in the same time. And, "generally, let D be the diameter of percussion, and W the weight, the power of any balance will always be as $D^2 \times W$, for the strength of the spring must be in this proportion to make it vibrate in the same time. If the number of vibrations be changed, that is, more or less in a minute, the velocity in similar parts of each vibration will be as this number, and if it be denoted by N, the power of any balance will be as $(N^2 \times D^2 \times W)$, the spring being in this proportion."

Mr. Nicholson, in the remarks he subjoins to this paper, says, that, admitting the inertia to be invariable, we deduce that gravitation varies inversely as the square of the distance between gravitating bodies; or that if the inertia be subject to variation, its changes are counterbalanced by such corresponding opposite changes in gravity, as produce a result of the same kind. But we seem to have no decisive facts to enable us to determine which of these hypotheses is the true one, and therefore prefer the simplest. He also observes, that the author of this communication appears to have confounded the terms gravity and inertia; but thinks the experimental speculation to which this paper relates is of sufficient importance to justify its publication.

Observations.—Whatever claim the anonymous author of this article may have upon the public for his ingenuity, he certainly has none either for his perspicuity and accuracy, or for his acquaintance with the subject he has chosen: and there is not an

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assertion in the whole paper better proved than that very modest one towards its conclusion, where he says, "I am very conscious, that my attainments in science are not such as to entitle me to be positive in a matter of this magnitude." And this is evident from his having confounded the three terms, gravity, weight, and inertia, and made use of the centre of percussion instead of that of gyration; all of which every person at all versed in natural philosophy knows to be palpably different. Indeed his attempt to shew that the force of gravity has a contrary effect upon the vibrations of a watch balance to that which it has upon the pendulum of a clock, has been altogether unsuccessful.

In the course of his reasoning, he observes, that "it is the inertia of the balance that regulates the spring; but its inertia arises from its weight." The first member of this sentence we readily admit; the last we can by no means grant. And as this is the hinge upon which his whole reasoning turns, we shall endeavour to shew, that the inertia of the balance is wholly *independent* of its weight.

In Atwood's Investigations for determining the Times of Vibration of Watch Balances, published in the Philosophical Transactions for 1794, it is demonstrated, that the resistance of inertia, by which the mass contained in the balance opposes the communication of motion to its circumference is $\frac{Wg^2}{r^2} = \frac{W}{r^2} \times g^2$; where W denotes the weight of the balance and the parts that vibrate with it, r its radius, and g the distance between the centre of gyration and the axis of motion. Now it is well known that the distance of the centre of gyration from the axis of motion is equal to the square root of the quotient, obtained by multiplying each particle in the balance by the square of its distance from the axis of motion, and dividing the sum of these products by the weight of the vibrating body: hence if W denote as before, and the sum of these products be represented by S, we shall have $g = \sqrt{\frac{S}{W}}$, or $g^2 = \frac{S}{W}$; which being substituted in the former expression for the inertia, gives $\frac{W}{r^2} \times \frac{S}{W} = \frac{S}{r^2}$ = the resistance of the inertia in general terms, totally *independent* of the weight of the balance.

Nor can we agree with the learned editor of the *Philosophical Journal*, in his annotations upon this paper, that it is still a matter of doubt whether or not the inertia of matter be subject to variation through change of place: and we trust, that the following reflections will shew that the invariableness of inertia is not *assumed* for want of proof, but *admitted* as being proved. If we consider any particle or portion of matter abstractedly from any fixed state, its tendency to preserve its present state, whether of

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motion or of rest, may be overcome, and a new state impressed upon it; and uniform experience has fully proved, that its resistance to any change from this new state to another is exactly equal to its resistance against a change from its former state to this, and from a third state to a fourth, provided an equal degree of velocity be either generated or destroyed at each change; and so on *ad infinitum*. So that, as long as a change is possible, and there are no limits, any particle of matter must resist *equal* changes *equally*; or, in other words, its inertia cannot be varied by any new force impressed upon it *ab extra*.

And as this resistance cannot be varied by any exterior power, so neither can it subsist in matter jointly with any other inherent quality of a contrary nature: for supposing it possible that two contrary tendencies could be implanted in matter; the one inducing it to change its state in any possible direction, and the other by which it resisted a change both in that and every other direction; and this supposition being admitted, these contrary tendencies must be either equal or unequal to each other. In the former case they would mutually destroy each other's effects, and leave no resistance at all; that is, annihilate even the very *nature* of matter: in the latter case, of unequal tendencies, one of them must prevail; and if it be the resistance to change, the contrary tendency will be completely destroyed, and the whole effect will be exactly the same as if their difference of power only had been implanted; but if the tendency to a change of state prevail, then, that matter should remain in any given *possible* state, would be a proposition *full* of contradiction and absurdity.

From this, therefore, it follows, that if the inertia of a particle of matter (and the same reasoning holds equally true for any assignable quantity) can neither be changed by any force impressed *ab extra*, nor subsist in conjunction with any other *inherent* quality of a contrary nature, we are justified in concluding, that it remains immutably the same, whatever may be the change of place. Nor do we deviate in the least from the *spirit of true philosophy*, when we admit the permanency of inertia as a *demonstrated fact*, rather than assume it as a matter that needs proof, according to the hypothesis of Mr. Nicholson.

Description of an improved Capstan, which works without requiring the Messenger or Cable coiled round it to be ever surged. Invented by J. Whitley Boswell, *Esq.—Trans. Soc. of Arts, Vol. XXV.*

As the method of hauling the cable on board large ships is perhaps familiar to but few general readers, Mr. B. in the first part of his communication to the Society, has given an account of this operation; and for the same reason we shall here follow his

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example as briefly as possible.—Large cables are not sufficiently flexible to admit of being coiled round the capstan, and therefore a smaller one, called *the messenger*, is employed for this purpose, having its ends connected together, and being fastened to the cable, as it enters the hawse-hole, by small ropes, called nippers, and relieved again as it approaches the capstan. And as the cable or messenger descends through a space on the capstan equal to its diameter at each revolution, a few turns bring it to the bottom; when it becomes necessary to loosen the coils in order to raise them higher, and this operation is termed *surging the messenger or cable*, and is always attended with delay, and sometimes with danger, to those performing it.

Mr. B. then describes three methods which had previously been invented to prevent surging. The first of them was by a horizontal roller placed beneath the messenger, to cause it to raise each coil above the capstan as the new one was formed. The second method of raising the coils on the capstan barrel was by a succession of upright lifters placed round the capstan, and made to rise as it turned round by means of an inclined plane. For this invention Mr. Plucknet obtained a patent, the specification of which was printed in the 46th number of the Repertory of Arts. The last of these methods was proposed by Captain Hamilton, an account of which is contained in the second volume of the Repertory: it consists in giving the capstan a conical shape, which renders the ascent of the coils easier, but causes them to loosen by the motion.

Mr. Boswell's method of remedying the defects of these methods is, by placing a smaller capstan parallel to the large one, and a little before it. The messenger or cable is passed successively round both these barrels or double capstan, but in an alternately reversed direction, so that the coils cross each other between the barrels, and come off at one end as fast as they are formed at the other. In order to prevent the friction that would arise from one part of the cable touching another in passing between the barrels, circular rings are fixed round each, at double the diameter of the cable and the thickness of the ring from each other; and in such a manner that each ring on one barrel is exactly opposite the middle of the space between those on the other. Mr. B. recommends that the projection of the rings be equal to the thickness of the cable or messenger; and that the capstan whelps be formed without any inclination, that the coils may have an equal degree of tension at both top and bottom. Each barrel should be furnished with catch-palls, and formed in other respects in the common way: the smaller need not much exceed half the height of the larger; a circumstance which will admit the capstan bars to pass over it in heaving round. Mr. B. also thinks that this principle might be applied to windlasses with advantage, by having a small horizontal barrel placed parallel to that of the

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windlass, and as much below it as circumstances would permit.

Besides its other advantages, indicated above, the construction of this improvement "is so very simple, that it is no more liable to derangement or injury than the capstan itself. Its cost can be but small, and every part of it can be made by a common ship-carpenter, and be repaired by him at sea if damaged by shot. It will take up but little room, only that of a half-barrel cask; and its nature is so analogous to that kind of machinery to which sailors are accustomed, that it can be readily understood and managed by them."

Observations.—Every improvement in naval affairs has long been regarded as an object which merited the attention of all who are sensible how much of our national greatness has sprung from this source; and our present political relations with every quarter of the globe have imparted to it a still greater degree of importance. Mr. Boswell, whose name is familiar to many of our readers, has been fertile in inventions relative to this subject; as may be seen by turning to the different volumes of this work: and his invention, which is the subject of the present paper, and for which the Society of Arts voted him their gold medal, is certainly not inferior to any of the others in either simplicity, ingenuity, or usefulness, or less worthy of general adoption. Besides avoiding the difficulty, danger, and delay of surging the cable or messenger, this double capstan is calculated to produce a considerable saving in the very expensive article of cables, by removing all lateral friction of the cable both against itself and against the whelps of the capstan. The application of this principle to windlasses, used at the mouths of coal-pits and mines for raising the coals or ore from the shafts, would prevent a considerable part of the injury which the ropes used for this purpose sustain, both from friction and from being kept so long stretched tight on the barrel on which they are coiled; as by this means the rope would be caused to pass off at one part of the windlass as it was wound on at the other; and thus, instead of the strain being imposed on the whole rope during the time that the weight is raised, and only taken off as it is uncoiled in the descent, it would be gradually relieved as it was drawn up.

It may be necessary just to add, for the information of such of our readers as are unacquainted with nautical terms, that by the capstan whelps are meant short pieces of wood, fastened on it to prevent the cable from coming too near in turning it round; and by the palls or pawls, short pieces of iron fixed to the deck, and resting upon the whelps, to keep the capstan from recoiling.

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Description of a portable Bridge, invented by Mr. JAMES ELMES, *Architect, of College Hill, Queen Street, Cheapside, London.—Phil. Mag. No.* 129.

THE component parts of this bridge are, a strong iron frame that constitutes the bottom, to which frames of the same metal are connected by hinges, for the convenience of packing, and are kept in an upright position when erected to form the sides by spring catches, fixed to a skirting of iron plate, fastened to the bottom frame; a square iron link, separated into two parts by a cross bar, each of which opens by means of a spring; and a kind of staple that opens and closes considerably at every revolution of a screw, worked by a small handspike.

As many of each of these three component parts are to be taken as are necessary to form a bridge of the required length; with planks, posts, and chains, according to local circumstances. From the small space in which those may be packed, Mr. E. says, a bridge of sufficient length for a considerable river will be very portable.

In the erection of this bridge, two strong posts are to be firmly fixed near the side of the river, at a distance from each other equal to the width of the bridge, which in this plan is nine feet; with smaller ones between these and the river. One of the bottom frames, with the sides attached, is then to be fixed to these posts, and to be supported by well-driven piles, or such other means as circumstances may admit. A second part is then to be connected to the first by passing one of the square links through the eyes at the bottom, and fixing one of the staples into the holes at the top of the upright frames on each side. Each succeeding part is similar, and is to be joined to the last in a similar manner; placing temporary supports under them where necessary until the opposite shore is attained, where the same process of fixing the posts, fastening the chains, &c. is to be repeated. The supports are then removed, and the staples screwed until the upper side of the bridge becomes level or attains a small degree of convexity, which will be sufficient to stiffen the whole, and occasion a small thrust outwards; it is completed by covering it with planks. And if the weight it is designed to bear should cause the middle to descend below the level, it is to be raised again by means of the screws at the top, as before.

A bridge of this description may also be rendered permanent, by fastening the parts firmly together, instead of connecting them as described above.

Observations.—In the actual erection of a bridge of this nature,

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we apprehend that considerable difficulty would be encountered in many, and even in most cases, in fixing the posts with sufficient firmness: and when it was of such an extent as to be of much utility, the danger of the chains at its extremities giving way would not be small—a circumstance that might probably be attended with fatal consequences. Nor are we convinced that this mode of constructing a bridge on a small scale, is superior to an old invention for the same purpose, described in a book published in 1588, entitled *Le diverse et artificiose Machine del Capitano*, by Agustino Ramelli; and referred to in our vol. ii. page 219. However, as the present circumstances of Europe have rendered inventions of this nature more than ever an object of public attention and utility, we hope Mr. Elmes's ingenuity will meet with its due degree of encouragement.

Account of an improved Machine for weaving figured Work. Invented by Mr. ALEXANDER DUFF.—*Trans. Society of Arts, Vol. XXV.*

As Mr. Duff's communication on this subject is not accompanied with either a written description or a plate, we can only refer those who may be desirous of becoming acquainted with the particular construction of this machine, to an inspection of the model deposited in the Society's repository, and present them with an account of the advantages that are to be derived from its use: and which will be best understood from Mr. Duff's own words:

"My machine is so contrived, that the most common capacity need never be at a loss to comprehend it. It is not so liable to go wrong as the old machines; it always keeps the figure in its form, which the others did not, and the weaver can do more work with it in ten hours than he could with others in twelve, besides the advantage of working with less labour; the back weight of six or seven pounds not being here wanted, the weaver has no occasion to press against his stomach, as one foot balances against the other. It regularly keeps its course without any attention from the workman, and as soon as one figure is finished, it begins of itself the next, whereas the old machines can only work one course, and must then be pulled back. In case the shoot breaks, or the quill runs out, the workman pulls a small cord, and treads back as many shoots as he thinks he has gone wrong; and on pulling another small cord, he goes on with his course as before; but in the old machines it must be set by the workman, and with considerable loss of time. It is not so injurious to the silk or to any other article of manufacture, as it does not break so much, one foot balancing against the other in

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reading, but the machines before in use are trod with one foot at a time, and have no guard. It is, besides, so constructed that it stands in less room, and is free from all noise in the working, and, moreover, it is less expensive than any yet contrived, the price of those in use being from three to five pounds, whereas mine can be made for two."

The names of seven gentlemen who have testified their approbation of this machine, are annexed.

Observations.—The same circumstance that has prevented us from giving a description of this machine, also deprives us of judging of its general construction, and of ascertaining whether the advantages, as stated by the inventor, are likely to be ensured by its particular mechanism. We can, therefore, only remark that its advantage over the machines previously in use for the same purpose in point of simplicity, the true criterion of improvement, seems to be distinctly pointed out by the difference in their prices; that the gentlemen who have signified their approbation of the machine appear to be acquainted with the manufacture for which it is intended; and that the Society for the Encouragement of Arts, Manufactures, and Commerce, rewarded Mr. Duff with a premium of fifteen guineas for his invention.

Description of a Goniometer, invented by the Rev. E. J. Burrow, *A. M. Fellow of Magdalen College, Cambridge.—Phil. Journ. No. 96.*

This instrument consists of a steel bar, about one tenth of an inch square, one end of which is chamfered off to a point; at the exact distance of an inch from which two other bars or legs are fixed to it by means of a screw or rivet, on which they turn as a centre. One of these legs is longer than the other, and it is brought to a point at the extremity to correspond with the bar. A movable quadrant passes through the bar, and is attached to the other ends of these legs, and is graduated on the side towards the shorter of them. A degree of longer radii may also be attached to the centre, for the purpose of reading off the smaller divisions with greater accuracy. The part of the bar between the centre and the point is divided into tenths of an inch; the two divisions nearest the point are also subdivided into twentieths, and these again into fortieths. The whole length of the instrument is about four inches.

If the crystal to be measured be a detached one, it is to be applied between the bar and the pointed leg of the goniometer, and the angle or its supplement is indicated on the graduated arch, according as it is acute or obtuse. But if it be imbeded in the matrix, any convenient distance is to be measured, by the

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scale of tenths, &c. on the instrument, from the angular point of the crystal; so that one of the pointed extremities of the goniometer being applied to the termination of this measure, and the other at an equal distance for the vertex on the opposite side, the angle subtended by the base of the crystal will be pointed out on the quadrant; and as its side is known, the angle at its vertex may be obtained by proportion.

This instrument may likewise be used in ascertaining the inclinations of strata in geological observations, by the addition of a small plummet suspended from the centre.

Observations.—This instrument is simpler in its construction than any other of a similar nature with which we are acquainted; and it appears to be capable of answering every purpose with equal ease and accuracy: besides possessing the additional advantage of being useful in geological observations.

On Deal Pendulum Rods. By Mr. Walker.—*Phil. Mag.* No. 129.

Mr. Walker's clock, to which a pendulum with a deal rod was applied, has a dead escapement, and goes when winding up. The daily rate of going was ascertained by a transit instrument, standing in the same room with the clock; and the observations were repeated at all convenient opportunities. It appeared to Mr. W. that clocks with pendulums of this nature gain most in the driest, and lose most in the dampest weather. He could not discover that either heat or cold affected the going of the clock, otherwise than it went very regularly during hard frost; which he attributes to the moisture in the pendulum being frozen. It also appeared that *moisture* did not affect the wood so much as the contrary extreme; the greatest rate of gaining, however, was only of short duration: hence he infers that wood imbibes *moisture* rapidly as soon as the dry season terminates. These remarks are accompanied by a table containing an abstract of the clock's going from 1798 to 1807.

Observations.—What advantage Mr. Walker expected to derive from the use of a pendulum rod made of deal in preference to one of metal, or what benefit he imagined the public would obtain from the publication of this paper, we are wholly at a loss to perceive; but it certainly requires neither penetration to discover, nor courage to assert, that they have received, they could receive *none*: and we should, therefore, have been glad to have seen a place, in the Phil. Mag. occupied with almost any thing else. [illegible] inaccuracy of pendulum rods of wood, with its causes,

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long been pointed out; and may be seen in the 14th vol. of the Repertory of Arts, First Series; as well as a series of experiments with a pyrometer on a piece of wood of the length of a seconds pendulum, which shews that the expansion in the length of the wood is irregular, and corresponds partly to the warmth and partly to the *moisture* of the atmosphere.

Description of a new air-tight Door-hinge, invented by Mr. MARTIN FURNISS.—*Trans. Society of Arts, Vol. XXV.*

THIS is an invention for uniting the joints of a folding screen or door, so as to permit them to turn either way without admitting the air to pass between them. The edges of the screen, &c. to be joined together, are to be made circular, and united by a piece of leather extending from top to bottom, and fastened on contrary sides, so that it may embrace the alternate parts of each curve. A chain, made of brass plates rivetted together, is also fixed at each end, winding round in a groove, in a similar manner to the leather, but in a reversed direction, which causes them to keep each other mutually tight. A plate of brass is likewise screwed over the joint at both ends, being fixed to the centres of the curves, in order to prevent them from being over-stretched, and to secure an easy motion of the chains and leather. A line of silk passes along the bottom of the screen, through a staple at each joint, for the purpose of keeping it air-tight on the floor.

Observations.—This contrivance manifests some degree of ingenuity in the inventor; and we do not perceive any thing in the construction which will prevent it from answering the purpose for which it was intended; and therefore think it worthy of adoption.

Proposal for altering the Scale of the Thermometer. By RICHARD WALKER, Esq.—*Phil. Mag. No.* 130.

MR. Walker, in this alteration, proposes that the numbers on his scale should commence at that point which indicates the temperature most congenial with the human body, in health, or that from which it is not conscious of experiencing any inconvenience from either heat or cold: and this he says is at 62 degrees of Fahrenheit's thermometer. Hence zero or 0 being fixed at this point, 150° on this scale will be the boiling point, and minus 30°, the freezing point of water; and all other degrees on Fahrenheit's scale may be reduced to the corresponding degrees on

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this, by subtracting 62 for any degree above 0 of the former scale, and adding the same number for any degree below it.

Mr. W. conceives that a scale of this kind will be sufficient for common purposes, which extends to 65 above, and an equal number below 0.

Observations.—As Mr. W. says, "The alteration I shall only briefly state now, reserving a fuller account of the reasons which induced me to adopt the scale I now propose, to another opportunity," we shall also defer extending our remarks on the subject till *another opportunity*; only observing that the public will doubtless think proper to be put in possession of Mr. Walker's "reasons" before *they* adopt his scale.

Description of a Watch Escapement, invented by Mr. S. MENDHAM.—*Trans. Soc. of Arts, Vol. XXV.*

MR. Mendham informs us that there are two principal and peculiar properties in this invention, both of which he considers to be superior to any thing of the kind previously laid before the public. These are, that the balance is kept in motion without friction; and that the motion is maintained by an impelling power without any blow whatever.

In this contrivance, the teeth of the balance-wheel are nearly similar to those of the balance-wheel of an eight-day clock, and move with the flat side forward against the pallet of an upright spindle. Above this pallet is another very small one, for the purpose of unlocking the wheel. This small pallet is kept against a locking spring, nearly similar to a detached one, by a hair spring fixed to the lower part of the spindle, with sufficient power to free the wheel, and permit the next tooth to fall upon the pallet, but so close home to the centre of the spindle that it has no power to pass it of its own accord.

The balance is fixed on the centre of its spindle for the purpose of equalizing the weight, and it is supported precisely over the spindle of the larger pallet, and the connexion between them is formed by means of two pins, one in each, which touch each other sideways; there is, therefore, no friction whatever between them.

The immediate cause of vibration is from the spring attached to the spindle of the large pallet, the balance-spring being so placed that the pin of the balance stands near the pin of the pallet; while the tooth of the wheel rests on the pallet during the vibration of the balance, so that when the balance is put in motion its pin comes in contact with the pallet pin, which stands perpendicularly, almost imperceptibly fine, and carries it back. As soon as

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it is moved, the tooth of the wheel gives it an extra assistance of about one fifth of a circle, passes, and lays the next tooth on the lock; on the return of the balance the pallet spring applies all its power in urging the balance forward till it arrive at the stop, when it maintains its motion, the small pallet unlocks the wheel, and its next tooth falls again on the great pallet, and waits the return of the balance.

Observations.—A portable machine which is capable of measuring time *correctly* has long been a very desirable object; but the difficulties to be surmounted in the construction of such a one are such as all the attempts of the most ingenious artists have not yet been able completely to overcome: very good approximations towards this point, however, have been the reward of their well-directed perseverance. The two greatest obstacles in this pursuit were the inequalities in the power communicated by the main spring, and the irregularities arising from friction and the variable resistance in which the balance moves. Both these, however, have been more effectually removed by Mr. Mudge in the invention of his escapement, which obtained a bounty from Government, than by any previous artist; yet from the great difficulty and expense with which it is attended, it is considered as still falling short of the desired object, and therefore, as leaving room for further improvement. This, Mr. Mendham conceives he has, at least in some measure, effected; and his description is accompanied with a letter from *Mr. Thomas Ramsay*, of Islington, drawing the following comparison between this escapement and Mr. Mudge's: "The impulse given to the balance without friction is exactly the same as Mr. Mudge's. The remontoir is bent up by the maintaining power in a similar way to that of Mr. Mudge's, but from the form of the pallet, which is a plain surface, it is not so perfect. Mudge's, from the form of the pallet, bends the remontoir always to the same place; the other is bent higher or lower according to the force of the maintaining power; but by forming the pallet like Mudge's, it would render them the same in this respect. The only other objection is the spring detent that detains the wheel, when it drops from the pallet of the remontoir; it is the same as that of the detached escapement, consequently exposed to the whole force of the maintaining power. To compensate for these objections, the arc of vibration is not limited like Mudge's, which is of great importance; and having one remontoir, it is more simple. It is, therefore, superior to Mudge's in having only one remontoir; and being unlimited in the arc of vibration, it is superior to the detached escapement in giving the impulse without friction." We have only to add that our opinion coincides in the main with Mr. Ramsay's.

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Letter on polygonal Numbers, in reply to Mr. Gough. By P. BARLOW, *Esq.—Phil. Journ. No. 96.*

AFTER making some remarks relative to the former part of this controversy, noticed in our two preceding numbers, Mr. Barlow observes, that Mr. Gough had evaded the only objection that affected the truth of the demonstration, by charging him with putting a false construction upon Mr. G.'s words. He therefore repeats the objection again, and remarks that, "unless it can be satisfactorily answered, the theorem will be still without a demonstration, however unwilling Mr. Gough may be to acknowledge it." Then, after a few remarks on the insufficiency of the examples which Mr. G. had adduced, he concludes with observing, that if Mr. Gough undertake to answer this objection, he shall leave it to the mathematical readers of the Philosophical Journal to determine how far it may be regarded as being satisfactory.

Observations.—As Mr. Gough has made no reply in either of the succeeding numbers of the Journal, it is not improbable, that, on reconsidering the subject, he has discovered the cause that induced Mr. Barlow to esteem his demonstration unsatisfactory. However, as Mr. G.'s ingenuity and talents are highly respectable, and his attainments in the analytical calculus, considering he is deprived of sight, truly astonishing, we trust he will neither relinquish the investigation of this curious theorem, nor despair of attaining that to-morrow in which he has failed to-day.

Account of Mr. CHRISTOPHER WILSON's *secure Sailing-boat, or Life-boat.—Trans. Society of Arts, Vol. XXV.*

THIS, Mr. Wilson has denominated a neutral-built self-balanced boat; and by the term *neutral-built* is to be understood one that is not built according to either of the methods previously in use, and technically called *clincher* and *carver*, but upon a kind of medium system, in which Mr. W. has endeavoured to unite the lightness of the one with the strength of the other. In this mode of building, the outside of the boat is smooth, and instead of the seams being caulked, they are covered on the inside with longitudinal slips, rivetted to each plank, with a sufficient quantity of blair made of tar and flocks, or any other caulking, between the slips and the planks, which keeps them water-tight and greatly strengthens the joint.

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With respect to the advantages attending this manner of building, Mr. Wilson states the chief of them as follows: "A boat of this construction has all the strength of one clincher-built, and can be made as light or lighter. It is free from the disadvantages of irregular outsides, and from the difficulty of repairing, which in this can be performed by any common workman in wood, as I have found by experience. A boat built this way has a fair and smooth outside, it has all the advantages of a carver-built one, at the same time that it is clear of the disadvantages of being loaded with unnecessary wood, which makes the carver work very heavy, the liability of leaks, and frequent want of caulking: There is one evil which both carver and clincher built boats have in common, that of having keel seams, and a vacancy between the sand or garboard streak and the upper part of the keel, which soon gets filled with dirt, and remains so, which naturally retains moisture, and speedily rots the wood. In this mode that evil is removed, by having the midship plank bolted on to the keel, wide enough to come over each side of the keel to clinch the slips on: this not only removes the evil, but saves a great deal of trouble in making the rabbets in the keel, and various bevellings in the sand streaks, which must be done by a good workman.

"These boats require no larger timbers than common clincher-built boats, as the timbers need no greater notches, but with this difference, that these timbers will catch the slips that are rivetted over the joints of the planks each way, and so the timbers and slips will brace one another, and add an additional strength; but in the clincher-built boats, the timbers catch the laps of the seams only one way, and consequently form no brace whatever.

"All I need explain further on the neutral system is its application. It can be applied to all open boats, of whatever form or use, to all coal and other barges, lighters, or any vessels used in rivers or canals, and also to all large cutters and luggers, which are now clincher-built."

The *self-balancing* property of this boat arises from two hollow sides, projecting about a foot in breadth at the middle, and gradually lessening towards each end, so as to preserve the regular shape of the boat. These are divided into several parts by means of water-tight partitions placed opposite each timber; so that if one or more of these parts should be broken by any accident and fill with water, the rest will keep the vessel buoyant. These partitions have lids at the top, for the purpose of admitting any thing that is wanted to be kept dry; "or part of them may be filled with cork shavings, and by that means, if the boat should happen to fill by accident, she cannot sink." These hollow sides, Mr. W. says, exclude a body of water nearly equal to a weight of two tons, which is much more than the weight of the

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men that will go in her: "and even if she had a hole in her bottom she would always keep a sufficient height out of the water for either rowing or sailing:" and in her fitness for both she is said to excel other boats.

"There is another advantage or property which this boat has: she cannot roll at sea, but always keeps a level position as far as the surface of the sea will allow; she may heel but not roll, as the balances are always ready to catch either way, and the opposite one assists the other by its weight out of the water and gravitation; neither can this boat pitch like another, for the balance bodies being out of the water, and the breadth of six feet only in the water, it can only act with a gravity on the water, equal to a boat of the weight of six feet, but has the resistance of the water upward equal to a boat of eight feet wide."

Mr. Wilson claims two distinct improvements, viz. the neutral mode of building, and the application of the hollow sides or balance bodies. The first of these improvements relates to boats, barges, &c. in general: the other to such boats only as are designed for dispatch, safety, or pleasure.

This account of Mr. Wilson's is accompanied by a certificate from two gentlemen who had examined and made a partial trial of his boat; and also by a very favourable letter to the Society, relative to a trial of this boat made at Newhaven, when the sea ran very high and the breakers were tremendous.

Observations.—In order to enable such of our readers as are unacquainted with the technical terms of boat-builders, to appreciate the advantages of this neutral system, it may be proper to observe, that by *clincher-built* boats is meant such as have the edges of the planks thinned off, and made water-tight by lapping over each other, and being rivetted firmly together, without caulking, except in the huddings and keel seams only. This kind of boats is capable of being made much lighter than those which are built according to the other mode, and styled *carver-built* boats. In this last kind the outsides are smooth, and the edges of the planks are joined together, and the seams caulked; which requires both the timbers and the planks to be much stronger, that they may admit of this last operation; and which consequently renders boats of this construction both heavier and more expensive than those of the former kind: but they are less liable to be injured by accident, more easily repaired, and present a smooth surface to the water.

With respect to Mr. Wilson's claim to two distinct and separate improvements, viz. his neutral mode of building and his application of the balance bodies, it is our duty to state that he has been anticipated in them both. In June 1803, Mr. Boswell had a boat built, the internal framing of which was precisely the same as in this of Mr. Wilson's. And the only difference was,

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that, instead of having slips laid on the inside of the seams with blair or other caulking, they were rivetted on without, and the seams caulked. This boat was exhibited in London, and other parts of the kingdom, for about eighteen months in that and the following year; and a description of it published in No. 62 of the Repertory of Arts, New Series, and in our vol. iii. page 471. The second improvement claimed by Mr. W. is essentially the same as that for which Mr. Lionel Lukin obtained a patent in the year 1785, the specification of which was published in the Repertory of Arts, vol. iii. First Series.

Now as these facts were known to the public long before the Society of Arts rewarded Mr. Wilson with their gold medal in 1807, they either were or ought to have been known to both him and the committee who bestowed upon him this honorary reward. If they *were* known, for what was Mr. W. rewarded? Was it for the union of the two prior inventions? Or was it because he had honoured the Society with a model of this union? It could *not* be for the invention itself. If they were *not* known, are we not authorized to conclude, that the members of the committee have shewn themselves unqualified for the discharge of those duties they had taken upon them, by being unable to distinguish between what was new and what was old; between a real invention and what was sent to them as such, from not knowing what had been previously done on the subject upon which they were to decide?

The first account we recollect to have seen of an invention of a life-boat, or one that is not liable to be either overset or sunk, so important an object to maritime states, was that of M. Bernieres, exhibited at Paris in the year 1777; and tried in the presence of a vast concourse of spectators. Eight men went into the boat, and rocked it till it filled with water, and afterwards rowed it along the river in that state, without danger of sinking. M. Bernieres then ordered a mast to be erected in the same boat, when filled with water, and hauled down by a rope, fixed to the top of the mast till it touched the surface of the river, so that the boat heeled in a position to which neither winds nor waves could bring it; yet as soon as the rope was let go, the boat immediately recovered its equipoise. This experiment furnished a decided proof that the boat could neither be sunk nor overset, and that it afforded the greatest possible degree of security against accidents.

Letter from Sir THOMAS CLARGES, *Bart. of Sutton upon Derwent, to W. Annesley, Esq. on the Subject of Life-boats.—Phil. Journ. No. 96.*

IN the first part of this letter, the writer makes some remarks on Mr. Lukin's patent boat and the Shields life-boat; and gives

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a brief history of the invention of the latter. He also takes a view of each as to its fitness for the intended purpose, and points out several objections to both, previously to giving an account of one of his own, built by Mr. Towill, of Teignmouth, and intended to possess the advantages of each, without any of its contrary qualities.

The leading features of this boat, Sir T. Clarges states to be these: "That she will not upset, or sink, or be water-logged; that she will afford cabin-room; that she is like a man of war's launch, well built for rowing, the oars not on a curve, but nearly in a right line, and low to the water, of which she draws little, a circumstance that renders her a fine pilot-boat."—The following description will probably enable our readers to judge of the manner in which these desirable objects are attained:

The boat is 30 feet long, 10 feet wide, and 3 feet 6 inches deep. The space between her timbers is fitted up with fine wood, and caulked and paid, in order to prevent the water from lodging there. Her buoyancy arises from eight metal cases, four of which are fixed to each side; each of these is water-tight, and independent of the others. Their size is such as to exclude a quantity of water equal in weight to six tons; but all the buoyant parts of the boat, taken together, will support a weight of ten tons. These cases are decked over, and covered on the sides with pine, and open at the top, by means of water-tight lids, for the purpose of admitting such articles as are wished to be kept dry. Her keel extends nine inches below the garboard-streak; it is narrow at the bottom, but wide at the top, which affords a greater security to the timbers when it is necessary to encounter sand-banks.

In order to add to the advantage of steering, the rudder of this boat is made to extend a foot below the keel; but it is so constructed, that it will easily draw up level with it when the water is shallow, and as easily let down again when it becomes deeper.

The boat is prevented from being water-logged by the following simple means: As the small space in the midships, where the rowers sit, is the only place where the water that is received over the sides can lodge, the bottom of this space is made higher than the surface of the water without, and two port-holes are made in the stern, to open outwards like valves: at these the water that enters is discharged, and that from without is prevented from entering by its pressure against the valves.

The forecastle contains a cabin ten feet wide, six feet long, and four feet deep, and supplied with air by means of two ventilators. It is designed for the better conveyance of women, children, and disabled persons; and is furnished with grapnels for throwing on board a wreck to ride by, the ropes of which will assist the sufferers to attain the boat with more ease. This boat is equipped with masts and sails; but in tempestuous weather is to be rowed with double-banked oars sixteen feet in length; and

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the men fastened to their seats, to prevent them from being washed overboard by the waves.

The author also observes, that "a boat of this kind, but of somewhat smaller dimensions, would be exceeding useful to ships on voyages of discovery, and indeed to any large vessels; as it would not only answer for wooding and watering, but is peculiarly adapted for excursions up rivers or small inlets of the sea, and exploring clusters of islands.

"As a pleasure-boat she answers remarkably well; and with respect to her safety, I can say, that I have sailed in her from Brighton, round the Cornish coast, to Conway in North Wales, without any accident, though we experienced some very dreadful weather on the voyage."

Observations.—On a comparison, this boat will be found to possess some advantages over Mr. Wilson's, described in the preceding article; though one of less dimensions would be more manageable, and in most cases preferable for the purpose of a life-boat. The cabin will doubtless prove serviceable in the protection of half-drowned women and children, or disabled persons of any description; and the mode of discharging the water that breaks in, by means of exterior valves, is extremely simple, and we believe entirely new: and when we reflect on the great number of lives that are annually lost on the shores of this kingdom, we regret that boats of this nature are not more numerous than they really are. It would certainly be advisable to build all pleasure-boats upon this construction, were it only for the idea of the safety that it affords, but doubly so on account of its fitness for preventing numerous accidents that so frequently occur among parties of pleasure.

Account of a Machine for cleansing Chimneys, without the Use of Climbing Boys; invented by Mr. Joseph Davis, *Kingsland Road.—Trans. Soc. of Arts, Vol. XXV.*

The upper part of this machine consists of five brushes fixed to a short tube about three inches in diameter. Four of them are fixed towards the bottom of it by means of hinges, for the purpose of cleansing the sides of the chimney, and are caused to expand to chimneys of different sizes by four springs. The head of each is about six inches long and five wide, and forms a portion of a cylinder, the hair being longer at the top than at the bottom, and still longer at the ends, for extending into the angles of the chimney. The fifth is fixed into the top of the tube by a spring and socket, and is intended for cleansing the pots at the top of the chimney: the machine, however, may be used either with or without it, or it may be used alone for cleaning stove-

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pipes, &c. The side brushes are kept close by drawing a cord, that the machine may be forced up the chimney with greater ease and facility; and expanded by another cord when it is at the top of the flue. The remaining parts of the apparatus are a number of rods, each having a brass socket at one end, with a notch or hole through it, and a spring on the other end with a small pin on its outside, which, in joining the rods, rises through the hole in the socket and retains them together. The rods are made of hiccory wood, as being most flexible and best adapted for the purpose.

The method of using the machine is by first entering the upper part of it, with the brushes closed and fixed on one of the rods, up the chimney; a second rod is then fixed, and the whole raised higher, and so on in succession until it reach the top. The string extending from the brushes to the bottom is then to be pulled, which causes them to expand to the width of the flue; and being then drawn down, they bring with them the soot which adhered to its sides. The rods are separated, as the machine is caused to descend, by means of a proper key.

Observations.—The machine here described, we doubt not, may be made use of with no inconsiderable success, as a great proportion of the whole number of chimneys may certainly be well cleansed with it: we, therefore, hesitate not to recommend it to general adoption, as a means of relieving a great number of children, who are employed in climbing chimneys under the inspection of unfeeling masters, from that state of misery and wretchedness to which they are reduced by such a disagreeable and sometimes dangerous operation; and we hope the public will be disposed to welcome the inventions, and reward the laudable endeavours, of those who apply themselves to so benevolent a purpose, as that of diminishing the number of infant victims which fall a sacrifice to the hardships and inhuman treatment they suffer in such a barbarous employment; and thus restoring a considerable number of fellow-creatures from the lowest possible state of degradation, of which this country admits, to their natural rank in civil society, by rendering the occupation either altogether, or in a great measure, unnecessary.

Description of an Invention to secure the Pannels of Doors and Window-shutters from being cut out by Housebreakers. By Mr. Joseph Davis.—*Trans. Soc. Arts, Vol. XXV.*

Mr. Davis's method of accomplishing this object is by introducing tempered steel wires through the stiles and pannels of the window-shutter or door, at the distance of about three inches from each other; which not only adds to the security, but also

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contributes greatly to the strength of such door or shutter. Mr. D. likewise sent the frame upon which the shutter was laid to be bored, along with his model, to the Society; and which has two upright pieces of wood or metal on one side of it, through which the borer passes in order to cause it to act in a proper direction on the pannel when it is laid upon the frame.

Observations.—This is a very simple, easy, and doubtless, in most cases, a very effectual method of preventing the evil, and reducing the number of depredations committed in this manner; and it merits general adoption.

An Example of the Utility of a Series in finding a Fluent. By a Correspondent.—Phil. Journ. No. 98.

The fluxional equation proposed is $\int x\,y\,\dot{x}\sqrt{(\dot{x}^2+\dot{y}^2)}=m\,x\,\dot{y}$, and y being put $=a+bx^2+cx^4+\ldots$, b is found, by a method which has no particular novelty, $=\frac{a}{4m}$, $c=b^3+\frac{b}{16m^2}$, $d=2b^5+\frac{5b^3}{18m}+\frac{b}{576m^2}$, and $e=56b^7+\frac{35b}{32m}+\frac{41b^3}{1152m^2}+\frac{b}{36864m^3}$. And when we have a particular determination, such as $\int x\,y\,\dot{x}=n\,x$, we may obtain an equation between x and a, whence either of them may be found from the other; that is, $\frac{1}{2}ax+\frac{1}{4}bx^3+\frac{1}{6}cx^5+\ldots=n$: hence, if x becomes very small; $a=\frac{2n}{x}$; and if a is evanescent, $\frac{a}{2}\left(x+\frac{x^3}{2\times 2^2m}+\frac{x^5}{3.2^2.4^2m^2}+\frac{x^7}{4.2^2.4^2.6^2m^3}+\ldots\right)=n$.

Observations.—We do not altogether approve of the obscure and enigmatical manner in which this anonymous author has brought forwards his investigation: by giving his paper a mathematical title, he has called the attention of mathematicians to a subject which is of little or no mathematical importance. We have, however, read this little essay with no inconsiderable interest, as we happen to have perused the work which seems to have given rise to it. The problem, of which a complete and general solution is here given, appears to have been abandoned by Mr. Laplace, as presenting some insuperable difficulties: this celebrated mathematician having only obtained, in his *Théorie de l'Action capillaire*, of which we have already given our readers an account, some very imperfect approximations. Mr. Laplace has considered the depression of mercury in very narrow and in

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wide tubes: when we examined his essay, we imagined that he meant to include all tubes whatever in the two descriptions of narrow and wide, since he has said nothing of any others; hence we were very naturally led into a misconstruction, which we have already corrected. But we have the satisfaction of finding our general opinion of the inaccuracy of Mr. Laplace's approximation, confirmed by the correct solution now afforded by this author, supposing, as we are inclined to do, that he has committed no error in his calculations. Thus for tubes six tenths and four tenths of an inch in diameter, Mr. Laplace calculates the depression .0038, and .0136 respectively, while the experiments of Lord Charles Cavendish had given .005 and .015; and the present author concludes that the numbers ought to be either accurately or very nearly .0044, and .0152; and his method is so much more direct than Mr. Laplace's, that it seems to be far less liable to error.

This anonymous author has also mentioned, as a solution of a particular case of his general problem, a formula, which had before been made public in a former number of this work, and which we think he must have seen before he wrote his paper: it is true that he does not claim any originality in this case; but it might perhaps have been more candid to have referred to the Retrospect for the assistance which he had derived from it.

Practical Rules for finding the Strength of Timber. Deduced from Dr. YOUNG's *Lectures. Communicated by a Correspondent.—Repertory of Arts, No.* 81, *Second Series.*

I. To find the number of hundred weights that will break a beam of oak supported at both ends.

Multiply the square of the depth in inches by one hundred times the breadth, and divide by the length.

Notes. 1. From $\frac{1}{4}$ to $\frac{1}{5}$ of this weight may be safely supported in practice.

2. If the weight be equally divided throughout the beam, the strength will be doubled.

3. Fir is probably a little weaker than oak.

II. To find how much a beam of oak or fir will sink when loaded in the middle.

Multiply the cube of the length in inches by the given weight in pounds, divide by the cube of the depth, and ten million times the breadth.

Notes. 1. This sinking is independent of any fixed setting of the wood, which will often be occasioned by it: and the irregularity of the wood will often cause the sinking itself to be at least twice as great as the calculation.

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2. The rule is the most correct for fir; oak sinks a little more.

Observations.—These rules are probably as correct as the nature of the subject would admit, and the latter we believe to be new, at least in its present form. But as we regard the former as insufficient to afford a practical man as much information relative to the comparative strength of timber as may enable him to work with judgment in common cases, we shall add the results of a few experiments on the subject.

A beam supported at both ends on two props will carry twice as much when the ends beyond the props are kept from rising, as when it rests loosely on the props, and will sink only one fourth as much with an equal weight. And when compressed endways, its strength is diminished; but when stretched in the same direction, it is increased.

The strength of a beam when the weight is suspended in the middle between the two supported ends, is to the strength of the same when the weight is laid nearer one end than the other, as the square of the whole length to four times the product of the distances of the weight from each end.

The strength of a rectangular beam, in an inclined position, is to the strength of the same beam in an horizontal position, to resist a vertical pressure, as the square of the radius to the square of the cosine of elevation. And any rectangular beam will support a greater weight when either of its diagonals is in a vertical position than when it is placed flat on one of its sides.

When a beam, the vertical sides of which are parallel planes, is fixed at both ends, it will be equally strong throughout, if either the tops of those sides, or the bottoms, or both, be terminated by ellipses.

The strength of a cylindric beam is to the strength of one whose section is its circumscribed square, as $\frac{1}{4}$ of the area of the circle to that of its circumscribed square. Hence, when the strength of the one is known, that of the other is easily ascertained. It may be of some practical utility to observe that the strongest rectangular beam that can be cut out of a given cylinder, is that of which the squares of the breadth, and depth, and the square of the cylinder's diameter, are respectively as the numbers 1, 2, and 3: and the ratio of the breadth to the depth will be that of 1 to $\sqrt{2}$, or nearly as 5 to 7, or still more nearly as that of 12 to 17. And if the diameter of the cylinder be denoted by d, the side of a square beam cut from it would be $d\sqrt{\frac{1}{2}} = \frac{1}{2}d\sqrt{2}$. Its solidity would be to that of the strongest beam cut from the same cylinder as $\frac{1}{2}d^2$ to $\frac{\pi}{3}d^2\sqrt{2}$, or as $\frac{1}{2}$ to $\frac{1}{3}\sqrt{2}$, or as 5 to 4·714045; while its strength would be to that of the strongest beam, as $(d\sqrt{2})^3$ to $d\sqrt{\frac{1}{3}} \times \frac{2}{3}d^2$, or as $\frac{1}{4}\sqrt{2}$ to $\frac{1}{3}\sqrt{3}$, or as

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3960 to 3849. This, however, supposes the fulcrum to be at the surface of the beam instead of being at the middle, as it actually should be: and therefore in that case it is not strictly accurate, yet it is sufficiently so for any practical purpose.

We have seen it stated, though we do not recollect precisely upon what authority, that the strength of a beam of oak is to that of a beam of fir, of the same dimensions, as 9 to 8.

An Essay on Sir Isaac Newton's Demonstration of the general Property of the Lever, with an Application of the same Principles to the Case of parallel Forces acting on a straight Lever. By OLINTHUS GREGORY, *L.L.D. of the Royal Military Academy, Woolwich.—Repertory of Arts, No. 82, Second Series.*

AT the commencement of this essay, Dr. Gregory remarks, that the importance of establishing the fundamental principles of mathematical and mechanical science upon unobjectionable grounds, and demonstrating its leading theorems with perspicuity and simplicity, has induced several eminent philosophers to direct their attention towards the attainment of a satisfactory, yet not complicated demonstration of the general property of the lever. But the demonstrations which have been offered at different times, from the first advances of mechanical science to its present state, have all been objected to by one author or another. As the demonstration of our illustrious countryman, Sir Isaac Newton, appeared to Dr. G. to be more simple, more obvious, and more satisfactory than any other with which he was acquainted, he has presented it to the public in a shape rather different from any in which it had previously appeared; that he might "more readily shew its application to the case of parallel forces acting on a straight lever (an application which it has been supposed it would not admit, except by a bare induction), and more easily defend it against the most plausible objections."

Besides the knowledge of the celebrated theorem of the *parallelogram of forces* and of statical composition and resolution, the demonstration of Newton requires the admission of only two principles, which may be stated in the form of lemmata, thus:

Lemma 1. In every system of an invariable form we may suppose powers or forces to be applied, without changing the effects at any points whatever in the lines of their directions.

"This proposition," the author observes, "is so evident as to be adopted without either proof or illustration, by many writers on the theory of mechanics. Indeed, it must be very manifest, that (admitting the convenience of application in all to be alike), if any body be acted upon by a force pushing against it by means of an inflexible bar, the effect will be the same upon the body at whatever point of the bar the force be exerted, the directions of

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the bar and of the force coinciding; and when the body is acted upon by a straight inextensible cord, the effect will be the same at whatever part of the cord the soliciting force acts: it being supposed in either case that the force is not employed in part in supporting the bar or cord." After proceeding a little farther in confirmation of this, the author states his

Lemma 2. Equal and contrary forces acting perpendicularly at the extremities of two equal arms of an angular lever (or two equal radii of a wheel), will prevent it from turning upon its fulcrum or centre of motion.

"This," he observes, "is a legitimate deduction from the doctrine of moments, in which it is affirmed, that the sum of the moments of forces which tend to produce rotation in one direction, is equal to the sum of the moments of forces which tend to produce rotation in a contrary direction, when the forces are in equilibrio. For the forces being equal, and the perpendicular distances of their directions from the fixed point being equal also, their products, that is to say, the moments, are equal likewise, and the equilibrium obtains." In order to satisfy the more scrupulous reader, Dr. G. subjoins a simple and perspicuous demonstration of this lemma, and then proceeds to apply Newton's demonstration to both bent and straight levers, in the three following propositions:

Proposition I. Any two forces acting upon a bent lever in different directions, but in the same plane, will be in equilibrio, if they are to each other reciprocally, as the perpendiculars let fall from the fulcrum upon their directions.

Proposition II. If parallel forces, acting perpendicularly upon a straight lever, keep it in equilibrio, they will be to each other reciprocally, as the distances from the fulcrum at which they act.

Proposition III. Any parallel forces acting upon a straight lever, will keep it in equilibrio, if those forces be to each other reciprocally as the distance from the fulcrum, measured on the lever, at which they act.

As each of these propositions naturally includes two cases: when the forces act on different sides of the fulcrum; and when they act on the same side of it; Dr. G. adapts two figures to each proposition, and demonstrates them in a very satisfactory manner.

The objections which have been urged against Newton's demonstration are then considered, and he conceives they may all be reduced to three principal ones, which he hopes will be deemed of little weight when the preceding lemmata and propositions are duly considered. These three objections are, 1st, The principle of equal forces acting perpendicularly, but in contrary directions, at the extremities of equal arms of a lever and producing an equilibrium, cannot be admitted, since

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we are supposed to be totally ignorant of the effects of weights or of forces upon any lever;—this, the author of this essay regards as an unnecessary supposition; as it is only some such preparatory knowledge as this that can, in general, induce a student to undertake the investigation of that invariable relation which subsists between forces acting upon a lever at any distances from the fulcrum, and in any directions. Besides this, every theorist who would enter upon this investigation, *must* be supposed to possess a previous acquaintance with the usual theorems relative to the composition and resolution of forces; and therefore be prepared to admit the truth of the second lemma. He ought likewise to be aware that these theorems are extremely confined in point of utility, while they are restricted to forces acting simultaneously upon a physical point; and that their most essential service in the science of mechanics arises from this circumstance, that if several forces act upon different parts of a body at the same time, and keep it in equilibrio, they are such as would maintain the equilibrium when acting at the same point, provided their directions continue respectively parallel.

2dly. Dr. Hamilton has objected to Newton's demonstration because it depends upon this supposition, that, when from the fulcrum of a lever several arms or radii issue out in different directions, all lying in the same vertical plane, a given weight will have the same power to turn the lever, from which ever arm it hangs, provided the distance of the line of its direction from the fulcrum remains the same. Dr. G., however, observes that the inadequate conceptions arising from confining the attention to one kind of force only, viz. weight, would be avoided by substituting for it the consideration of forces in general; and that Dr. Hamilton's objection is removed by the first lemma. He also adds, "Indeed it is not a little curious that Dr. Hamilton's own reasoning seems to contain a reply to the objection he advances:" and then obtains, from a demonstration founded upon the same reasoning, two equal rectangles, which are "obvious measures of equal tendencies to produce rotation."

3dly. It has been objected to Newton's demonstration, as wanting *universality*, in not being applicable to parallel forces acting upon a straight lever; this objection is removed by the second and third of the foregoing propositions.

Observations.—As this theorem is one of the principal of those which constitute the basis of mechanical science, a short account of what has been done in endeavouring to demonstrate it upon clear and self-evident principles, will probably be acceptable to a great part of our readers; we shall, therefore, subjoin a brief sketch of some of the chief demonstrations that have been pub-

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lished, with the leading objections that have been urged against them.

The first we believe who attempted it, at least whose demonstration has been handed down to us, was *Archimedes*. His demonstration is founded upon the supposition, that if a number of *equal* weights or bodies be suspended from the arms of a lever, at *equal* distances from each other, their tendency to cause the lever to turn about its fulcrum will be the same as it would be if they were all united, and suspended from a point which is situated in the middle between all the points of suspension, and considered as the common centre of gravity of all the bodies. This supposition, however, is by no means self-evident, and therefore the investigation founded upon it has been rejected by succeeding mathematicians as imperfect.

The next demonstration of this general property, in point of time, was that given by *Galileo*, in his second dialogue on the resistance of solids. This demonstration for the straight lever is more simple and obvious than that of Archimedes, but as it depends upon the noted theorem concerning the common centre of gravity of two bodies, which itself needed demonstration, as it was only inferred from the general property of the lever, it was not without its objectors.

Mr. Huygens, in his *Miscellaneous Observations on Mechanics*, observes, that some mathematicians have endeavoured, by changing the form of Archimedes's demonstration, to render its defect less sensible, but without success. He then proposes one of his own, in which he takes for granted that, when two equal bodies are suspended from the arms of a lever, that which is furthest from the fulcrum will have the greater effect in causing the lever to turn on its fulcrum. To this it has been objected, that it is resting the demonstration upon a fact that is to be proved by it; and even independent of this irregularity, on which we should not lay much stress, his demonstration is very tedious and prolix, and holds only when the arms of the lever are commensurable.

Maclaurin's demonstration, given in his *Account of Newton's Philosophy*, is also considered as imperfect; as he has inferred the truth of this general property from an inductive process, and has not extended it to the case where the arms of a lever are incommensurable.

The demonstration given by *Mr. Landen*, in his *Memoirs*, appears to be founded upon self-evident principles, and his reasoning upon them correct; but as the investigation is long and tedious, and includes several cases, it has been regarded as wanting a sufficient degree of simplicity to render it proper for an elementary treatise of mechanics.

Dr. Hamilton, in his Essays, after objecting to Newton's demonstration, as well as to several of those noticed above, proposes to supply the defects he had pointed out, by a demonstration

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of his own; which, however, depends upon this proposition, that when a body is kept in equilibrio by the action of three forces, they will be to each other as the three sides of a triangle parallel to their respective directions. With respect to this proposition, *Mr. Vince*, in the Philosophical Transactions for 1794, observes, "Now this is true, when the three forces act at any point of a body; whereas, considering the lever as the body, the three forces act at different points, and therefore the principle, as applied by the author, is certainly not applicable. If in this demonstration we suppose a plain body, in which the three forces act, instead of simply a lever, then, the three forces being actually directed to the same point of the body, the body would be at rest. But in reasoning from this to the case of the lever, the same difficulties would arise, as in the proof of Sir I. Newton. But admitting that all other objections could be removed, the demonstration fails when any two of the forces are parallel. Another demonstration is founded upon this principle, that if two non-elastic bodies meet with equal quantities of motion, they will, after impact, continue at rest; and hence it is concluded, that if a lever which is in equilibrio be put in motion, the motions of the two bodies must be equal; and therefore the pressures of these bodies upon the lever at rest, to put it in motion, must be as their motions. Now this is comparing the effects of pressure and motion, the relation of the measures of which, or whether they admit of any relation, we are totally unacquainted with." Mr. Vince then proceeds to complete the demonstration of Archimedes, by proving the principles upon which it depends. However, we do not think that this demonstration yet possesses that high degree of clearness and simplicity which the learned Professor seems to attribute to it.

Dr. Hutton, in his *Mathematical and Philosophical Dictionary*, article LEVER, observes, that writers on mechanics commonly demonstrate the general property of the lever in a very absurd manner, viz. by supposing the contrary: that is, they demonstrate that a body is at rest by supposing it to be in motion. He then adds, "I shall therefore give here a new and universal demonstration of the property, on the pure principles of rest and pressure, or force only." This new demonstration, however, is essentially the same in principle as Dr. Hamilton's (a circumstance with which we presume Dr. Hutton was unacquainted); and is, therefore, liable to the same objections.

D'Alembert, *Prony*, and *Fossenex*, have likewise given demonstrations; but they cannot be regarded as free from objections, though we have not room to dilate upon them here.

To the ingenious and learned author of the present Essay, the demonstration of Newton appeared to deserve the preference; and he informs us that "the professed objects of this paper are to defend the principles adopted by Newton from objections, and to

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shew that they apply to straight as well as to bent levers." We think he has not laboured in vain. Indeed, his application of these principles to the straight lever seems so simple, obvious, and satisfactory, that it may be thought rather extraordinary it has never been pointed out before, by those who have previously treated on the same subject. On the whole, we are very glad to see our illustrious and incomparable countryman so zealously and ably defended.

Proposed Improvements of the Hygrometer. By J. BERZELIUS. *From his Philosophical Journal, for* 1808.

MR. Berzelius first makes some observations on Mr. Dalton's ingenious researches relative to the quantity of water contained in the atmosphere, and on Mr. Dalton's table for shewing the quantity of water capable of maintaining a gasiform state, correspondent with every degree of the thermometer, apparently with a view of shewing the usefulness and necessity of the hygrometer in experiments of this nature. He then remarks, that "the hygrometer should express to what column of mercury the aqueous vapour of the air corresponds; and at the same time determines the absolute quantity of the vapour, and, the temperature of the air being known, how much of this vapour it can take up beyond what it already holds, and how soon the exhalation thereof can take place. Our usual hygrometers of hair, and of whalebone, are, in this respect, very imperfect: the results from them are not much to be relied upon, having always a relation to the temperature in which the examination is made."

The simple instrument used by Mr. Dalton, in his Hygrometrical Essays, consisting of a long cylindrical glass filled with cold water, and its mode of operation in precipitating the water from the atmosphere, are then noticed; the temperature of both the water and the atmosphere being determined by the thermometer. But as this method was somewhat slow and troublesome, Mr. Berzelius thought of effecting this precipitation by the thermometer itself. For this purpose he caused an oblongated thermometer bulb to be made of steel; to this bulb, a tube of the same metal about an inch in length was attached, into which a glass thermometer tube was closely cemented. The bulb is to be covered with an oil-case, and immersed in water, or any cooling mixture, till the mercury has fallen to the required degree of temperature; and then being taken out, the bulb becomes covered with a quickly-passing coat of aqueous vapour; and the expansive capacity of this vapour is indicated with the greatest precision by the scale prefixed to the tube. Besides the nicety of the results obtained by this apparatus, it also possesses the advantage of ad-

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mitting an experiment to be made in less time and with less trouble than by the former methods.

Observations.—We should have been glad if Mr. Berzelius had mentioned the thermometer he used in his experiments, as, without a knowledge of this, little benefit can be derived from his account. We however apprehend that the Swedes make use of the centigrade thermometer; and especially as, on turning to Svanberg's "*Exposition des Opérations faites en Lapponie, pour la Détermination d'un Arc du Méridien, en* 1801, 1802, *et* 1803," we find it is there used.

De Luc has shewn that all animal and vegetable substances, from their natural structure and organization, are subject to contrary changes in the direction of their fibres from different variations of humidity; and consequently that the accuracy of their indications cannot be depended upon for hygrometric purposes. These substances, however, are not all affected in an equal degree by the same change; and De Luc has made choice of whalebone in his hygrometer; Saussure adopted a horse-hair. Lieutenant Henry Kater afterwards invented a new instrument for the same purpose, uniting a greater degree of simplicity and accuracy than either of the other two (see our vol. iii. page 341): in this he has preferred as a hygroscopic substance, the beard of the grass called, in the Canarese language, *Obeena Hooloo*, and very common in the Mysore country. The quantity of moisture in the atmosphere is pointed out in this instrument by an index, with all the accuracy of which the hygroscopic substance is capable: how far this substance may exceed whalebone and horse-hair in its suitableness for the intended purpose, or whether it exceeds them at all, we are not prepared to say; it is certainly inferior in this point to Mr. Berzelius's hygrometer, provided the principle upon which this last is founded can be proved, viz. that the atmospheric vapour is *independent* of the air. Mr. Berzelius asserts that this has been shewn by Mr. Dalton, while others affirm that it is not demonstrated by any thing that gentleman has done.

Historical Accounts of the Steam-engine, extracted from a Treatise of Mechanics, theoretical, practical, and descriptive, by Olinthus Gregory, *Vol. II.; and the Edinburgh Review, No. XXVI. Art. III.*

The vast extent and importance of this subject, together with the great and numerous advantages which this country derives from this master-piece of mechanical invention, have induced us to go a little out of our usual course to bring the matter before our readers, and we have therefore prepared an abridgment of

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Mr. J. C. Hornblower's History of the Steam-engine, inserted in Mr. Gregory's Treatise on Mechanics; and of the account which the Edinburgh Reviewers, in their criticism of Mr. Hornblower's treatise, have also given of the invention, in the last number of that Journal.

1. *Mr. Hornblower's account of the Steam-engine.*—After remarking that it is unnecessary to dwell upon the early part of this history, already treated by Desaguliers and his followers, Mr. Hornblower observes, there are, however, some particulars during this period which deserve notice. One of these relates to Desaguliers's account of the method of leathering the piston, in which Mr. H. thinks he has erred in stating it to have been discovered by accident. Another respects what he says of an experiment made by Mr. Beighton, with a view of ascertaining the comparative magnitude of such steam as was generally used for working an engine, in respect to the quantity of water from which it was produced, and which is stated to be in the ratio of 13,338 to 1; but which Mr. H., from Mr. Beighton's statement, calculates at 2,655½ to 1; and he considers even this to be more than what may be observed in some of the most improved engines of the present time. He also conceives that experiments of this nature and date have either been conducted or related in so loose and inaccurate a manner, that little dependence can be placed upon the results.

Friction and inertia were at this time considered as the two grand objects which the engineer had to overcome: and some different modes of condensation were tried, but without much advantage being derived from them. The water in the piston, whenever it was a tight one, became hot, and a considerable degree of heat was expended in its evaporation. Several improvements, however, were attempted in the construction of boilers, some of which succeeded and are still in use: but the grand obstacle was to be surmounted by Mr. Watt. Previous to Mr. Watt's improvements, the boring of the cylinders had been executed in a very imperfect manner; and in several of those engines in which the water obtained from the mines was used for condensing the steam, this defect soon became so much increased by its corroding properties, that it was almost impossible to keep the piston tight, and in some cases the packing was rammed so hard as to sustain the whole pressure of the atmosphere.

Such was the general state of this engine when Mr. Watt obtained his patent in 1769, and engaged to grant licences for the use of his improved engines, on the condition of receiving one third of the advantage which should result from the saving of fuel in working those of his construction. Much of the merit ascribed to Mr. Watt, for the improvement in boring the cylinders, Mr. H. says is really due to Mr. Wilkinson, with whom it, as well as most of what related to the iron-foundery, originated. The method of condensing the steam in a separate vessel is likewise said

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to have been discovered by a Mr. Gainsborough, about the time that Mr. Watt was engaged in bringing forward his improvements, and communicated to Mr. Watt by an officious acquaintance of Mr. Gainsborough.

An attempt had been made before this time to drain some of the deep mines in Cornwall, by a new application of Savary's engine, furnished with an apparatus for opening and shutting the usual communications by means of cocks and valves. In this application it was proposed to employ the force of steam expansively, with a stratum of air between it and the water, to prevent its condensation; and Mr. Blakey obtained a patent for his improvements: but one of the steam-vessels bursting before the steam had attained a sufficient power for the intended purpose, shewed the impracticability of the scheme, and it was abandoned.

Mr. Hornblower thinks that those who have attempted to estimate the defects of Newcomen's engine have erred in stating the vacuum to be such only as would cause a weight to be raised of about 7½ pounds for every square inch of the piston; and says that the column of water alone was equal to that weight independent of both friction and inertia, which were very considerable; he also adds, that he tried the vacuum of several engines in Cornwall, and found that which was least to bear a load of 11.6 pounds on the square inch. Another error has been committed with regard to the purport of the counter-weight attached to the outer end of the lever, which has been stated to be employed to overcome part of the pressure of the atmosphere at the return of the stroke of the engine: "*that is not the case.*" It is used for the purpose of regulating the speed of the engine's working, and both the weight and the end of the lever on which it is placed are to be varied by the attendant according to circumstances; the three principal of these that require to be attended to in this regulation, are, "first, that the pump-buckets shall descend, but without such force as may endanger the breaking of the pump-rods; secondly, that this descent shall nevertheless be as quick as possible; but, thirdly, that it shall not impede the discharging functions of the engine." With respect to the application of steam, the old atmospheric engine possesses one advantage of which Mr. Watt's single engine does not admit. In pumping water from mines it is necessary that the speed of the engine should be regulated according to the influx of the water; and in the old engine this is effected by merely regulating the intensity of the fire, as by this means the force of the steam on the lower side of the piston may be varied from an equilibrium with the pressure of the atmosphere to exceed it by two pounds on each square inch of the piston, which, in a cylinder of 60 inches in diameter, amounts to a force of 7,200 pounds. This advantage, however, relates only to the working of pumps; for since its application to the purposes of giving motion to mill-work, it has

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been a desideratum to maintain a uniform force or action on the crank, in order to produce the same effect on the fly. When the engine was first applied for the purpose of producing rotative motion, it had not the advantage of a double stroke, as at present; and the mode of equalizing the strokes was by a rod which connected the engine and the mill together, or a weight laid on that end of the lever. This mode, when applied to the old engine, required that the work to be performed by it was nearly of a uniform nature during the time of its action. But Mr. Watt accommodated this circumstance in his single engine by rendering the discharge constant, and not liable to be affected by any variation in the resistance, and checking the entrance of steam by a contrivance that prevents the plenum valve from opening to its greatest extent. The first notice, however, of a rotative motion being communicated by the steam-engine, was about the year 1778, when Mr. Washbrough obtained a patent for that principle, and applied it at his own works for turning lathes, and other purposes.

The valve by which the air is discharged, and called the blowing valve, Mr. H. says, was not applied to any of Mr. Watt's engines previous to his going into Cornwall, as before that time this operation was usually performed by a temporary brake attached to the discharging pump. This valve was first applied by Mr. Hornblower at an engine on a mine called Ting Tong, which he erected for the *proprietors*, and not for Messrs. Boulton and Watt, as has been stated.

The most novel circumstance in the operation of Mr. Watt's single engine, and which is a fine accompaniment of the improver's principal object, is, that when the steam has acted on the piston to the limits of the stroke, it is permitted to re-enter the cylinder below the piston. "It generally happens in engines erected for pumping water, that they are calculated to go deeper than the present bottom of the mine; and therefore, if all the steam which enters the cylinder for one stroke was to be condensed, the engine would act with its whole power, and the effect would be to destroy itself: on which account, in engines thus circumstanced, the injection is to be stopped long before the termination of the stroke, which leaves a residuum of steam at the bottom of the cylinder, that proves an effectual banking to the piston; even so far as to support it when the chains to which it is appended have become quite slack by the momentum given to the lever during the action of the steam on the piston. We believe it was this circumstance that indicated to Mr. Watt the advantage of shutting off the steam from the boiler soon after the commencement of the stroke."

As the valve which must be opened at each succeeding stroke of the engine, in order to secure its action, is kept down by a weight equal to the pressure of the atmosphere, added to the elasticity of

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the steam above that pressure; and it is necessary that this valve should be opened as quickly as possible; in large engines it requires a considerable force to effect it in such a manner as not to impede the performance of the engine. Mr. Hornblower says that it was suggested to Mr. Watt to make the valves double, by placing a small one in the middle of the larger, and it was adopted; but the difficulty of keeping them in complete repair caused the method to be given up. In all the best engines a weight or spring was applied for the purpose of opening this valve. But Mr. Jos. Hornblower is said to have constructed this valve on a new principle, in order to effect this purpose more completely, and his mode is here preferred to any that had been previously in use.

Such was the state of Mr. Watt's improvements and of his single engine, when new wants gave rise to new inventions. Some of the Cornish miners wished to carry their works to a greater depth than could be conveniently done by the engines then in common use, and Mr. Watt invented his double-stroke engine. In the single engine, the piston is connected to the lever by chains lying on the arch of the inner end, but in this it must be connected by a mode that will render the rod rigid in its action upward; and this Mr. Watt has effected by a most ingenious system of transverse joints which compels the rod to a motion parallel to itself. At the other end of the lever a rod connects the motion of the engine to a fly, by the application of one wheel fixed on the axis of the fly, and another on the rod that is connected to the lever. But, as simplicity is always a desideratum in the construction of machinery, Mr. Hornblower gives the preference to a simple crank with a fly of such weight as may have the required momentum with a less velocity.

A patent was taken out in 1781, for an improvement on Mr. Watt's single engine by Mr. Jonathan Hornblower of Penrhyn. This improvement consisted in obtaining a greater power by a complicated force of the steam than could be obtained by its simple action in the common mode. This is effected by the use of two cylinders of different capacities. And Mr. H. after inquiring into the effect of using steam according to each of these modes, compares the results together as follows: "If we obtain the accumulated pressure by taking a mean of the extremes, we shall find Mr. Watt's application to be $\frac{24+24+12}{3}=20$, leaving 12lbs. at the termination of the stroke. The application of the principle in the present instance, by taking the mean of the extremes, will be $\frac{24+18}{2}=21$, leaving 18 at the termination of the stroke; which in point of advantage in favour of the double cylinder, is as 6 to 2, a point of no small magnitude in the practical

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application of this principle, and which seems to have been overlooked by all those who have taken up the subject." Mr. Hornblower is here stated to have entered upon this project in the year 1776, and continued it until he had made a large working model, in which the cylinders were 11 and 14 inches in diameter; and that Mr. Watt's use of the expansive valve had never been put in practice until long after Mr. Hornblower had projected the design of his double engine. This gentleman also had another patent granted him for an engine having a rotary motion within itself, by the immediate action of steam on four revolving pistons, mounted on an arbor with a hollow axis.

The two improvements in the engine invented by Mr. Edmund Cartwright, are a tight piston and a condenser from which the atmospheric air is excluded. This last is made of as thin copper as the nature of its application will admit, and a large external surface is exposed to the water in order to keep it at a low temperature, so that when the steam comes in contact with it internally the condensation may be produced. An engine on this principle erected at Horseley-down, is said to give great satisfaction to its proprietor, and to perform its operations very smoothly and effectually.

Mr. Hornblower then concludes his subject with observing that "Mr. Watt's engine, as it now stands, is the work of six-and-thirty years, and we may hold it as complete in its kind as it possibly can be. It has exercised the ingenuity of the inventor, besides frequent accessions from the ingenuity of other men; various pretensions and conceits no doubt will abound to rival its excellency, and time only, the arbiter in human affairs, will determine their fate. We would rather see a laudable competition prevail to simplify its parts, without affecting the principle, either by reducing their number, or by dispensing with their costly finish, or both, that it may come within the compass of the middle ranks as well as the more opulent; and the man who sets the example will deserve well of his country."

A communication from Mr. Farey, junior, occupies the next place in the treatise from which the above account is taken. This is a descriptive account of a steam-engine, erected in 1802 by Messrs. Murray and Wood of Leeds, who obtained a patent for some improvements in what are technically called the *noxles* or *steam-valves*, and for opening these valves by a circular motion derived from the rotary motion of the engine-shaft in the preceding year (see Repertory of Arts, vol. xvi. p. 298, of the First Series). Mr. Gregory has then given some account (according to M. Prony), with a description of all the parts that are peculiar to one of the simplest of the engines of double effect which were made by Bettancourt, after he had visited this country, and had been permitted to examine in part the exterior of the engine at the Albion Mills, constructed by Messrs. Boulton and Watt,

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and from whose chief improvement his does not differ much in its essential principles. The author of the treatise has also added an account of the late alterations made in this engine by Mr. Woolf. But for what relates to this gentleman's inventions we shall refer our readers to the first and second volumes of this work, pp. 79 and 107.

2. *The Edinburgh Reviewers' Account of Steam-engines.*—The honour of inventing the steam-engine is ascribed to the Marquis of Worcester, as the first idea of it is found in a small work, entitled "*A Century of Inventions,*" published by him in 1663, and consisting of brief accounts of a number of schemes, relative to inventions and improvements, which had at various times presented themselves to his mind. What relates to his contrivance, which has obtained the appellation of steam-engine, is very short and obscure; and all that can be obtained from it is, that he had actually had a machine constructed for raising water by means of steam; but in what place or manner this was effected is probably not to be ascertained. It is supposed, that the force of this engine was derived solely from the elastic power of steam, and that the condensation of steam by cold constituted no part of his invention. This last is attributed to Captain Savary, who had erected several engines previous to the year 1696, when he published a small tract, entitled "*The Miner's Friend.*" In his engines, the alternate condensation and pressure of the steam took place in the same vessel into which the water was first raised by the pressure of the atmosphere, and then expelled by the elasticity of strong steam.

The next who effected any essential improvement in this engine was Newcomen; and for which he obtained a patent in 1705. This consisted in causing the steam to act in different vessels from those in which the water was raised; and employing the weight of the atmosphere for the purpose of pressure only, while the air was displaced by means of steam, and a vacuum produced by condensation. This was no small improvement, as it enabled him to make use of steam of much less elasticity, and therefore to work with less heat, which produced a considerable saving in the expense. To him, this engine is indebted for the introduction of the steam cylinder and piston, their connexion with the pump by means of the main lever with its rods and chains, and several other inventions of less importance.

In this state, however, the engine required the constant attendance of a man to open and shut the cocks by which steam and cold water were alternately admitted, until Mr. Henry Beighton, in 1717, invented the means, or at least perfected the mechanism, for making the engine perform this operation itself. Several other of its parts were also much improved by him. No further improvement of consequence was made in the structure of this engine until the year 1764: it still continued to be styled

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Newcomen's, or the atmospherical engine; and was still subject to many imperfections. The steam was condensed in the cylinder; the hot water was expelled by the force of the steam; the piston was forced down by atmospheric pressure, and was kept tight by being covered with water; the injection cistern was considerably elevated, in order that water might enter with greater force. Experience had proved that the engine could only be loaded with about seven pounds for each square inch of the piston, and the great difference between this weight and that of the atmosphere was supposed to be occasioned by friction. The quantity of fuel necessary to evaporate a given quantity of water, and the quantity of steam produced from it, were alike unknown; and whether the heat of steam corresponded exactly to its temperature, as well as the proper quantity of injection-water, for a cylinder of certain dimensions, had not been determined.

Mr. Watt, at that time a mathematical instrument maker at Glasgow, undertook the repair of the model of a steam-engine of this nature belonging to the university of that city; and in the course of his trials with it, he discovered that it required a greater quantity of both fuel and injection-water in proportion than larger engines. In order to ascertain the cause of this difference, and remedy these defects, he made many experiments relative to the best materials for making cylinders; the means of producing a more perfect vacuum; the heat at which water boils under different degrees of pressure; and the quantity of water necessary to produce a given bulk of steam under the ordinary pressure of the atmosphere. These points, as well as the quantity of fuel requisite to evaporate a given quantity of water, and the quantity of cold water to be injected at each condensation of the steam, being determined with a much greater degree of precision than before, the cause of the defects in Newcomen's engine became evident. "It appeared that the steam could not be condensed so as to form an approximation to a vacuum, unless the cylinder, and the water it contained, were cooled down to less than 100°; and that, at greater degrees of heat, the water in the cylinder must produce steam, which would in part resist the pressure of the atmosphere. On the other hand, when greater degrees of exhaustion were attempted, the quantities of injection-water required to be increased in a very great ratio; and this was followed by a proportionate destruction of steam on refilling the cylinder." A consideration of these circumstances led Mr. Watt to conclude, that in order to obtain the most perfect vacuum with the least possible waste of steam, it was necessary that the cylinder should be brought to a temperature of less than 100°, and that no steam should be condensed in refilling it. Mr. Watt perceived that to effect this the cylinder must be kept always as hot as the steam by which it was filled; and that, by opening a communication between the cylinder when full, and another vessel (which he calls a con-

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denser) exhausted of air, the steam would rush into the condenser until the equilibrium was restored; and that if a sufficient quantity of cold water were injected into the condenser, the steam it contained would be reduced to water, and no more steam would enter until the condensation was complete. The condenser he emptied of its air and water by means of a pump, which is known by the common name of the air-pump. Some defects still remained to be remedied in Newcomen's cylinder, in which the piston was kept tight by means of water, some of which passed by the sides of the piston, and injured the vacuum by its evaporation: this water, as well as the atmospheric air, also reduced the temperature of the cylinder considerably. "Mr. Watt removed these defects, by applying oils, wax, and fat of animals, to lubricate his piston and keep it tight: he put a cover on his cylinder (with a hole in it made air and steam tight, for the piston-rod to pass through), and employed the elastic force of steam to press upon the piston; he also surrounded the cylinder with a case containing steam, or a case of wood, or of other non-conducting substance, which would keep it always of an equable temperature."

The improvements of this engine being carried to this length in Mr. Watt's mind, he executed a working model in the year 1769, which fully answered his expectations. It worked readily with a load of 10½ lb. for each square inch of the piston, and was even capable of raising 14 lb. per inch; and required only about one third of the steam that was necessary in the common atmospheric engine to produce the same effect.

Mr. Watt having erected an engine on a large scale for Dr. Roebuck of Kinneil, which confirmed his previous expectations, and in which the saving of fuel exceeded two thirds of what was used in Newcomen's engines; he then obtained a patent for his inventions in 1769; and Dr. Roebuck became associated in the prospects which it opened. Dr. R. however, soon disposed of his interest in the concern to Mr. Boulton, the founder of Soho manufactory; and the business of constructing steam-engines soon after commenced under the firm of Boulton and Watt. In reducing his inventions into practice on a large scale, Mr. Watt now made improvements in several of the parts of Newcomen's engine. He caused the cylinders to be bored with a greater degree of precision than had been previously done; "he adopted a new mode of constructing the piston and screwing down the packing, and secured the rod in the piston in a more perfect manner; he introduced puppet-valves into the steam-boxes or nozles, instead of the old sliding regulators; he used better means of opening those valves, and added various improvements in the working gear; he suspended the working beam, so that the centre of motion was below the centre of gravity, instead of being above, as in the old engines; and he improved the mode of

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setting the boilers on the grates, as well as the apparatus for keeping the boilers regularly supplied with water." He likewise used the steam in some of his early reciprocating engines to act expansively.

The next object that engaged Mr. Watt's attention was that of applying the power of steam to produce rotary motion; and for this purpose he took out a patent for a steam-wheel which he had invented; but this mode was abandoned from a persuasion that this motion would be better derived from the motion of the piston in the reciprocating engine. This kind of motion, however, had been obtained in an atmospheric engine erected at Hartley coalery, in Northumberland, as early as 1768. On one end of the beam was fixed a toothed sector, which worked into a trundle, and this last, by means of two pinions with rachet-wheels, produced a rotative motion in the same direction, by both the ascent and descent of the arch; and by changing the position of the rackets the motion could be reversed. This engine worked but very imperfectly.

In 1769, a patent was obtained by a Mr. Stewart, for an engine which produced a rotative motion, by a chain going over a pulley, and round to barrels furnished with rachet-wheels: a weight was suspended to the loose end of this chain, for the purpose of continuing the motion during the return of the engine. Mr. Washbrough's patent mode of communicating a motion of this nature, by the reciprocating strokes of the steam-engine, was virtually the same as had been previously used in the engine at Hartley, with the addition of a fly; which was now used for the first time, but which had been previously thought of by Mr. Watt. It is also said, that the idea of communicating rotative motion from the beam of the steam-engine, by means of a crank, had early occurred to Mr. Watt, but that he did not set about putting it in practice till the year 1778 or 1779, when he had a model made for that purpose, which performed to his satisfaction; and it is added, that a workman, who had been employed on the model, informed the persons engaged about one of Mr. Washbrough's engines of the contrivance. Mr. Watt then set about other modes of producing the same effect; " and, in 1781, took out a patent for several new methods of applying the vibrating or reciprocating motion of steam-engines to produce a continued rotative motion round an axis, one of which was that beautiful contrivance of the revolving motion of one wheel round another. This, however, was only part of what Mr. Watt saw to be necessary, in order to perfect this application of the steam-engine. The steam had hitherto been used only to press down the piston, which was returned by a weight at the opposite end of the beam, so that the power of the steam may be said to have been inactive during that period. Mr. Watt remedied this, by applying the power of the steam to press the piston down, as well as to press it up, thus

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forming alternately a vacuum above and below the piston. This he called *the double engine*; and, in fact, it doubled the power exerted within the same cylinder. He had long had in his mind the idea of this improvement; and had even produced a drawing of it to the House of Commons in 1774, at the time he procured the act to prolong his original patent; but the first he executed was, we believe, at Soho, in the year 1781 or 1782, and the first public exhibition of it at the Albion Mills a few years later.

About the same period, finding double chains, and racks, and sectors, very inconvenient for communicating the motion of the piston-rod to the angular motion of the beam, he invented and applied what has been called the *parallel motion*, one of the most ingenious and most perfect contrivances in mechanics.

To prevent irregularities in the speed of the engine, arising from the variations in the quantum of power used at different intervals in the works to which it was applied, he made an application of the centrifugal force of what is called the governor (before used in wind and water mills), to regulate the admission of the steam; by this means keeping the engine always at a uniform velocity, and diminishing the consumption of steam in proportion to the power exerted; thus giving the finishing stroke to the perfection of the motion of this machine, and rendering its regularity nearly correspondent with that of the pendulum of a clock."

Observations.—In the perusal of these accounts, our readers will perceive, that in the former of them are mentioned the inventions of Mr. Gainsborough, for condensing the steam in a separate vessel; of Mr. Blakey, for employing the expansive force of steam; of Mr. Jos. Hornblower, for a new construction of the valve which forms a communication between the boiler and the cylinder; of Mr. Jonathan Hornblower, for obtaining a greater power by a complicated force of steam in an engine with a double cylinder; and of Mr. Edmund Cartwright, for a vacuous condenser and an improved method of packing the cylinder, which are not noticed in the latter. In the last an account is given of the original invention by the Marquis of Worcester; the improvements of Captain Savary, Newcomen, and Beighton; and of the rotary motion at Hartley coalery, and of Stewart's patent method for the same purpose, which are not inserted in the former. Three of these, however, are supplied by Mr. G. in his introductory account.

Besides the difference in these accounts, relative to the inventions of Mr. Watt, Mr. Washbrough, and others, the Reviewers, in their animadversions upon "the view which Mr. Gregory and his associate have taken of the same subject," observe, respecting Mr. Gainsborough's invention, "it is quite impossible that Mr. Watt's idea of condensing in a separate vessel could be de-

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rived from that gentleman. Mr. Watt, while he resided at Glasgow about the year 1764 or 1765, invented that method of condensation. Mr. Gainsborough's improvement, whatever it was, was posterior by more than twenty years." Mr. Hornblower states, relative to the same subject, that Mr. Gainsborough's "model succeeded so well as to induce some of the Cornish adventurers to send their engineer to examine it; and the report was so favourable as induced an intention of adopting it. This, however, was soon after Mr. Watt had his act of parliament passed for the extension of his term; and he had, about the same time" (1774), "made proposals to the Cornish gentlemen to send his engine into that country. This necessarily brought on a competition, in which Mr. Watt succeeded." And he also adds, "it is well known that Mr. Gainsborough opposed the petition to the House of Commons through the interest of General Conway." How this statement, and the concluding sentence of the preceding one, can be reconciled with each other, we are under the necessity of leaving to our readers to determine: we, however, cannot perceive any reason why these inventions should not have taken place independent of one another.

When speaking of the engine for which a patent was taken out in 1781 by Mr. Jonathan Hornblower, of Penrhyn, the Reviewer says, "In the account, however, one circumstance is omitted, which is very material in the history of this engine, viz. that in the year 1799, it became the subject of an action, as an infringement of Mr. Watt's patent; and that the miner, who had used the engines of this construction, paid the portion of savings in fuel claimed by Messrs. Boulton and Watt for the use of their invention, rather than risk the event of a law-suit. It should, besides, be observed, that if this engine merits the eulogium bestowed upon it, it seems singular, that not one of the kind has since been erected, though all legal obstructions were removed, by the expiration of Mr. Watt's patent in the year 1800." He likewise observes, that the patent granted to Messrs. Murray and Wood in 1801, for their invention of the nozles or steam-valves, and the method of opening them, was set aside in 1802 or 1803, by a writ of *scire facias*, at the instance of Boulton and Watt. He then takes a view of the account of Bettancourt's engines, which Mr. Gregory has given from Prony's *Architecture Hydraulique*, and concludes that the credit of those inventions, which Mr. Prony has ascribed to Bettancourt, belongs "exclusively to Mr. Watt." It is also added, that what Mr. Gregory has said of Mr. Woolf's improvements appears to be unsatisfactory.

We have given this brief statement of the Reviewers' remarks on Mr. Hornblower's Historical Account of Steam-engines, and lament with them, that a man of Dr. Gregory's eminent scientific acquirements should, from any circumstance, have admitted into a work, in other respects highly

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valuable, a tract too rude, and in some parts too erroneous, to deserve a place in such a treatise. From our knowledge of Mr. Gregory's *character*, however, we conceive our duty compels us to observe that we believe his motives for inserting it were very different from those ascribed to him by the Reviewers.

We shall take this opportunity to state that Mr. Hornblower's calculations are not always correct. In his statement relative to Mr. Beighton's experiment for ascertaining the ratio between the quantity of steam, and that of the water from which it was produced, there appears to be an error in taking the content of a pint at 38.2 inches instead of 35.25 as results from $282 \div 8$: and the ratio ought therefore to be 2893 to 1 nearly; this calculation, however, supposes no steam to be condensed in refilling the cylinder, which was certainly not the case, as in the atmospheric engine a considerable portion of the steam was expended in this way. Mr. H.'s statement relative to the elastic force of steam acting against the under side of a piston of 60 inches diameter with a force of two pounds for each square inch of its surface is not correct: this he states at 7200 pounds, but $60 \times 60 \times .7854 \times 2 = 5654.98$ pounds only. Mr. H.'s account, however, is by no means destitute of practical information.

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1. *A concise Method of determining the Figure of a gravitating Body revolving round another. By a Correspondent.—Phil. Journ. No.* 88.
2. *Calculation of the direct Attraction of a Spheroid, and Demonstration of Clairaut's Theorem. By a Correspondent.—Phil. Journ. No.* 89.

1. In this investigation, the figure of the body is supposed to vary very little from that of a sphere, and each of its particles is conceived to revolve in an equal orbit, in order that the centrifugal force may be equal to the mean attractive force; then the difference between this mean force and the local attractive force will be proportional to the distance from a plane passing through the centre of the revolving body and perpendicular to the direction of the central body. The want of parallelism in the attractive force will also produce a second disturbing force, between which and the whole force the same ratio subsists as between its distance from the line which joins the centres of the bodies, and their distance. When each of these forces is reduced to the direction of the circumference of the sphere, it is found to be proportional to the product of the sine and cosine of the equatorial plane. "Thus the gravitation towards the moon at the earth's surface is to the gravitation towards the earth, as 1 to 79 times the square of $60\frac{1}{2}$, or to 256217, and the first disturbing force is to the whole of this as 2 to $60\frac{1}{2}$ at the equatorial plane; and the sum of both reduced to the direction of the circumference where greatest, as 3 to 121,

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that is, to the whole force of the earth's gravitation, as 1 to 10,334,000. And in a similar manner the joint disturbing force of the sun is to the weight as 1 to 25,763,000."

If a sphere be inscribed in an oblong spheroid, and the elevation of the spheroidal surface above that of the sphere be measured in a direction parallel to the axis of the spheroid, it will always be proportional to the sine of the distance from its equator. The inclination of these surfaces is here ascertained to be every where proportional to the product of the sine and cosine; and the tangent of this inclination, where greatest, to be equal to the ellipticity of the spheroid. If the density of the elevated parts be regarded as inconsiderable, and their attraction be neglected, and the ellipticity be to the radius as the disturbing force to the whole force of gravitation, an equilibrium will obtain. The density of the sea being supposed small in comparison with that of the solid parts of the globe, and the earth's radius being taken at 20,839,000 feet, the height of a lunar tide in this equilibrated state will be 2.0166 feet, and that of a solar tide equal to .8097.

The next subject of inquiry is the attraction of the elevated parts according to any supposed density. The mode of conducting this inquiry is, by supposing an indefinite number of parallel and equidistant circles to be described on the surface, commencing with the situation of any gravitating particle, and being divided by a plane which bisects the equatorial plane of the spheroid. Then, from the relation which subsists between the square of the distance of the gravitating particle from the equatorial plane, the sine of this distance, reduced in the ratio of the radius to the cosine of the angular distance of the circle from its pole, and the actual sine of any arc of the circle reduced to a direction perpendicular to that of the plane, the disturbing attraction of all the prominent parts, at any assigned point, is ascertained to vary as the product of the sine and cosine of the angular distance of this point from the equatorial plane; the quantity of matter only being variable, while the similarity of its arrangement remains constant. Hence, as the sine of these attractions at all the points, and the original disturbing force, both vary as the inclination of the surface, an equilibrium will be maintained between these and the tendency to descend towards the centre, when the ellipticity is duly commensurate with the density of the prominent parts.

The magnitude of the ellipticity corresponding to a given disturbing force, and a given density, is then subjected to investigation. Since the quantity of matter at each point is expressed by the product of the cosine and the square of the sine, and the immediate attraction of each particle, when reduced to the common direction, is as the sine directly and the cube of the chord inversely, the compound effect of these will be as the cosine, and the cube of the sine directly, and the cube of the chord inversely, or as the cube of the cosine of half the arc and the cosine of the

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whole arc conjointly. "If therefore we call the sine of half the arc x, the cosine of the whole rc will be $2x^2-1$, and the fluxion of the arc being $-\frac{2\dot{x}}{\sqrt{(1-xx)}}$, that of the force will be $\frac{2\dot{x}^3-4\dot{x}^5}{\sqrt{(1-xx)}}$, of which the fluent is $(\frac{4}{5}x^4+\frac{2}{3}x^2+\frac{4}{5})\sqrt{(1-xx)}$, as may be shewn by substituting, in the reduction of its fluxion, $\frac{(1-xx)}{\sqrt{(1-xx)}}$ for $\sqrt{(1-xx)}$: and while x decreases from 1 to 0 this fluent becomes $\frac{4}{5}$." It now becomes necessary to determine the unit with which this quantity is to be compared: and this is found to be equal to a cylinder, the diameter of which is equal to that of the inscribed sphere, and its height equal to the ellipticity of the spheroid; the content of this cylinder will, therefore, be to that of the sphere, as $\frac{1}{4}$ of the ellipticity to the radius: and when the densities are equal, the ratio is that of $\frac{1}{4}$, $\frac{4}{5}$, or $\frac{1}{5}$ of the ellipticity to the radius. The remaining part to be compensated is therefore $\frac{2}{5}$ of the whole; hence the ratio of the ellipticity to the disturbing force must be that of 5 to 2. "And if the density of the sea be to the mean density of the earth as 1 to n, the disturbing force, produced by its attraction, will be to the ellipticity as $\frac{3}{5n}$ to 1, and the primitive disturbing force as $1-\frac{3}{5n}$ to 1."

The heights of the solar and lunar tides, in a state of equilibrium, and upon the supposition that the density of the sea is inconsiderable, are found to be .8097 and 2.0166 feet respectively; but if the spheroid were homogeneous, they must be increased to 2.024 and 5.042 feet. The real value of n, however, is near $5\frac{1}{2}$, and of $\frac{3}{5n}$ nearly $\frac{1}{9}$; consequently the ellipticity is to the primitive disturbing force as 9 to 8, and the heights of the tides in equilibrium .911 and 2.269 feet, and their joint height 3.18 feet.

The same reasoning and calculation will also apply to an oblate spheroid: and in this case also the ellipticity is "to the disturbing force, where it is greatest, as 1 to $1-\frac{3}{5n}$, or to the centrifugal force at the equator, as 1 to $2-\frac{6}{5n}$. Thus, the centrifugal force being $\frac{1}{289}$, if the density were uniform, the ellipticity would be $\frac{1}{231}$; but since it is in reality about $\frac{1}{311}$, $2-\frac{6}{5n}=\frac{1}{289}$; and $n=1.32$, n implying here the mean density of the earth compared with the mean density of the elevated portion of the spheroid,

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which hence appears to be about three fourths of that of the whole earth."

Direct Attraction of a Spheroid.

2. THE same mode of calculation is also applicable to the magnitude of the immediate attraction, or to the determination of the comparative length of a pendulum on different parts of the spheroidal surface. If we conceive a sphere to be inscribed in the spheroid, and another to be circumscribed about it, the attraction at the pole will be equal to the attraction of the inscribed sphere increased by $\frac{4}{15}$ of that of the shell, and at the equator equal to the attraction of the circumscribed sphere decreased by $\frac{2}{15}$ of that of the same shell. The surface of this shell is equal to the curve surface of its circumscribing cylinder; and the attraction of a narrow ring of the equatorial part of the spheroid may be expressed by its breadth; but its attraction in the direction of its axis is reduced in the ratio of the cube of half a right angle to the cube of the radius: and that of any other ring will be to this in the joint ratio of the cube of the cosine of half the angular distance from the pole and the versed sine. Then if the attraction of the shell be denoted by 2, the cosine of half the angle by x, "the versed sine being $2-2x^2$, and the fluxion of the arc $\frac{2\dot{x}}{\sqrt{(1-xx)}}$, the fluxion of the force at the equator will be $\frac{1}{2\sqrt{2}} \cdot \frac{2\dot{x}}{\sqrt{(1-xx)}}$ and elsewhere as much less as $x^3(2-2x^2)$ is less than $\frac{1}{2\sqrt{2}}$, that is, $\frac{4\dot{x}x^3}{\sqrt{(1-xx)}} - \frac{4\dot{x}x^5}{\sqrt{(1-xx)}}$, of which the fluent is found as before $(\frac{4}{5}x^4 - \frac{4}{5}x^2 - \frac{8}{15})\sqrt{(1-xx)}$: and this becomes $\frac{8}{15}$ while x increases from 0 to 1, being to 2, the attraction of the shell, as $\frac{4}{15}$ to 1; but if the radius of the sphere be 1, and the ellipticity e, the attraction of the shell will be to that of the sphere as $\frac{3e}{2}$ to 1, n being the mean density of the sphere, compared with that of the superficial parts, and the attraction of the spheroidal prominence will be expressed by $\frac{4e}{5n}$, that of the sphere being unity.

"The depression below the circumscribed sphere is equal, on the meridian, to the elevation above the inscribed sphere; but vanishes at the equator, being every where proportional to the square of the sine of the latitude; so that the mean depression of each of an infinite number of rings, of which any point of the equator is

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the pole, must be half as great as the elevation of the corresponding rings parallel to the equator; and the whole deficiency is equal to half of the whole excess, that is, to $\frac{2e}{5n}$: consequently, the remaining attraction of the shell is $\frac{13e}{5n}$, from which we must deduct the diminution of the attraction of the inscribed sphere $2e$, and the whole will become $1 + \frac{3e}{5n} - \frac{2e}{n}$, which subtracted from $1 + \frac{4e}{5n}$, leaves $2e - \frac{9e}{5n}$ for the excess of the immediate attraction of the pole above the equatorial attraction; to which if we add the centrifugal force f, the whole diminution of gravity g will be $2e - \frac{9e}{5n} + f$; but since e was before found to f as 1 to $2 - \frac{6}{5n}$ or $= \frac{5n}{10n-6} \cdot f$, we have $\frac{10n-9}{5n} \cdot e = \frac{10n-9}{10n-6} f$; and $g = \frac{20n-15}{10n-6} \cdot f$, to which if we add e, we find $e + g = \frac{25n-15}{10n-6} \cdot f = \frac{5}{2} f$; and this is the celebrated theorem of Clairaut."

The remainder of this paper is occupied in shewing that the diminution of the attractive force at different parts of the spheroid varies as the square of the cosine of the latitude.

Observations.—The natural connexion of the subjects involved in these investigations appears to be such as to justify the two papers being classed together in the same article. The importance in which they have been held by modern philosophers, and the immense ingenuity, labour, and learning, which have been manifested in their solutions by so many of the eminent mathematicians since the days of Newton, have induced us to give as full an account of these two papers as our limits would allow, and to adhere as strictly as possible to the words of their author. The number and prolixity of those investigations, however, render it impossible for us to enter into a sufficient account of them to enable our readers to determine the comparative merits of the present article: we shall therefore only offer a few general remarks relative to what has been done, and refer those who are interested in the subject to the authors themselves.

The figure of a gravitating body, in the circumstances in which it is supposed to be in these investigations, depends upon the law of gravity at its surface; and to determine the ratio which subsisted between the primitive gravity of a particle situated at the pole, and that of a similar particle at the equator, in direct and

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general terms, appeared, even to Newton, to involve such difficulties, that he avoided it by taking an indirect method; and succeeding mathematicians have considered his solution as being liable to reasonable objections. Stirling gave some propositions in the Philosophical Transactions, for 1735, which tend to illustrate the theory of Newton. The investigations on this subject, published by M^c^Laurin, in 1742, in the second volume of his Fluxions, manifest a very superior degree of ingenuity and geometrical elegance, and apply to spheroids of any ellipticity. The elaborate performance of M. Clairaut, published in the following year, and entitled *Théorie de la Figure de la Terre*, will long remain a monument of his eminent skill in researches of this nature. Euler and Daniel Bernouilli, in the Memoirs of Berlin and Petersburgh, and d'Alembert, in his *Recherches sur différens Points importans du Système du Monde*, have all handled the subject with great learning and acuteness. Lagrange, in the Memoirs of Berlin for 1773, Laplace, in the *Mémoires de l'Académie*, for 1772, and Legendre in those for 1784, have each treated on the subject. But we conceive that the most complete solution that has yet been effected, is that in Laplace's *Mécanique Céleste*. Considerable information on the subject may likewise be obtained by consulting Thomas Simpson's Dissertations, the Geometrical Dissertation by F. Boscovich, the Cosmographia of Frisius, and the fourth volume of Montucla's *Histoire des Mathématiques*.

The late Professor Robison, in his *Elements of mechanical Philosophy* (following the method of Boscovich), has obtained very simple expressions for the gravitation of a particle, situated on the surface of a spheroid, differing but little from a sphere, and in similar circumstances to those in the present investigations, except that the density is supposed to be uniform. The expression for the gravitation of a particle in the pole of the spheroid to all the matter by which the spheroid exceeds the inscribed sphere is $\frac{8}{15}\pi \times Ee$, where π denotes the periphery of a circle, of which the radius is 1, and Ee the difference of the radii of the inscribed and circumscribed spheres; and for the gravitation of a particle situated at the equator to all the matter by which it falls short of the circumscribed sphere is $\frac{4}{15}\pi \times Ee$. Hence the primitive gravity at the pole may be obtained by adding the first of these expressions to the gravitation to the inscribed sphere, and that at the equator by subtracting the last of them from the gravitation to the circumscribed sphere. In general terms, if g denote the mean gravity, P the polar, and e the equatorial gravity, r the radius of the inscribed sphere, and x the elevation of the equator above the inscribed sphere; we have this general proportion of the primitive gravitations, $P : e :: r + \frac{1}{5}x : r$, or (because x is very small in respect to r), $P : e :: r : r - \frac{1}{5}x$. This last is generally the most convenient, and it is exact, if r be taken for the equatorial radius. The same author adds, "had the spheroid

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been prolate, the same reasoning would have given us $P : e :: r : r + \frac{2}{5}x$; and that the gravitation at the pole of an oblong spheroid, the gravitation at the equator of an oblate spheroid (having the same axis), and the gravitation to the circumscribed sphere, on any point of its surface, are proportional, respectively, to $\frac{1}{3}r + \frac{1}{15}x$; $\frac{1}{3}r + \frac{2}{5}x$; and $\frac{1}{4}r + \frac{1}{7}x$."

The author of the two papers which constitute the present article, after noticing the prolixity of preceding calculations on the subject, adds, "I shall endeavour to point out a method of treating it, which is extremely compendious and convenient." We, however, conceive it to be our duty to remark that, while we are ready to allow that his investigations possess the former of these qualities, apparently, we are fully persuaded that every one who has bestowed half as much attention upon them as we have done, is thoroughly convinced that it is principally in appearance; and that it has in some measure been obtained at the expense of perspicuity. Indeed, in order to obtain this seeming conciseness and simplicity, many important steps in the investigation are omitted, and much is left for the reader to prove before he can be satisfied of the truth of what is advanced. We hope that a writer of so much ingenuity, as the author of these papers certainly is, will in future bear in mind that perspicuity stamps the highest value upon every investigation, and that whatever is obtained by the sacrifice of this is too dearly bought. We are also of opinion, that the papers ought to have been accompanied by diagrams, which might perhaps have tended to remove a part of that obscurity of which we complain. At the same time we must do the author the justice to say, that, as far as we know, he is the first who has calculated the true elevation of the tides in the actual conditions of the comparative density of the earth and sea.

REVIEW OF SPECIFICATIONS OF PATENTS,

PUBLISHED IN THE REPERTORY OF ARTS, MANUFACTURES, &c.

During the Months of January, February, and March, 1809.

Mr. RICHARD FOTHERGILL's *Patent for a Machine for dressing Hemp, and making and spinning the same into Ropes and Cordage. Dated April* 1793. *Term expired.—Repertory of Arts, No.* 80, *Second Series.*

THE machinery invented by the patentee for accomplishing the purposes stated in the title to this specification, is of such a complicated nature that it is impossible to give a particular description of it without a plate. That part of it which relates to dressing the hemp consists of a combination of fluted rollers, and wheels set with teeth similar to those of the common hackle, by the re-

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volution of which the hemp is prepared for spinning, and transferred to the other part of the machinery. In this last the process of making the rope commences; the yarns being first spun and received on bobbins, the strands are then formed and wound on reels, each of which revolves on its own axis, fixed to a platform, which also turns on its axis, and carries the strand-reels round with it. From these reels the three strands unite at a point above them, and are there twisted into a rope, which is drawn away as it is made by the revolution of another wheel adapted for the purpose. The whole machinery is put in motion by any power given by hand, horse, water, or steam, or by any other means.

Observations.—The origin of the art of rope-making, as far as it relates to the application of twists in contrary directions, for the purpose of preventing the yarns which form their constituent parts from opening out, must undoubtedly be referred to a very remote period; but all improvements either in the process or in the quality of the manufacture, are of recent date. The first attempt to improve the process was made by a Mr. Sylvester, who sent a model of the machine which he had invented for that purpose, to the Society of Arts, prior to the year 1783. This machine was somewhat similar to the latter part of Mr. Fothergill's apparatus, but less perfect; and we believe it was never carried into actual practice. In 1784, a patent was granted to Mr. Benjamin Seymour, for a method of substituting horses instead of men for moving the machinery at the head of a ropery, but it was not found to answer the intended purpose. In 1792, the Rev. Edmund Cartwright obtained a patent for a machine for making ropes. This machine was similar in principle to Mr. Sylvester's, but more perfect in its construction. The next attempt was towards improving the principle of this manufacture, for which Mr. John Daniel Belfour, of Elsineur, obtained His Majesty's letters patent in March 1793; and the specification may be seen in vol. ii. of the Repertory of Arts, First Series. The next invention was this of Mr. Fothergill, which besides that of preparing and spinning the hemp, was an improvement upon the inventions of Mr. Sylvester and Mr. Cartwright. By this invention the whole process of rope-making was reduced to a much greater degree of uniformity, and rendered capable of being performed without the use of a rope-ground, and with considerably less labour. The complicated nature of this machinery, however, was a considerable check upon the extent of the manufacture by this mode, until further improvements were made which tended to introduce a greater degree of simplicity into both the construction and operation.

We have noticed these few inventions because they have served as the basis of most of the other improvements that have since

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been made in a branch of manufacture, so important to this country, as may be seen by consulting Mr. Chapman's "Treatise on the progressive Endeavours to improve the Manufacture of Cordage," or the different volumes of the Repertory of Arts.

Mr. JOHN DICKINSON'*s Patent for the Invention of a Cannon Cartridge Paper, manufactured on an improved Principle.—Dated November* 1807.—*Repertory of Arts, No.* 80, *Second Series.*

MR. Dickinson's invention consists in adding a certain proportion of wool or woollen rags to the linen materials previously used in the manufacture of this kind of paper: and the intent of this addition is to prevent the paper from retaining sparks of fire after the flame goes out, when lighted by the explosion of the powder in firing the gun. Mr. D. directs that the proportions of the mixture be two fifths of woollen, and three fifths of linen materials; or the quantity of woollen may be either increased or diminished according to the strength of the linen with which it is mixed: as the greater its proportion is, the more completely will the paper be prevented from hanging fire, but its strength will be thereby reduced. Each kind of materials is to be made into half stuff separately, and then mixed and beat off together in the same beating machine. "The paper should be 'engine-sized' with alum only, in the proportion of about ten pounds to one hundred and twelve pounds of stuff, and no oil or spirits of vitriol, or any other ingredients, should be put into the engine." It is then to be cured by the usual method.

CHARLES GRANT *Viscount de Vaux's Patent for a Machine, which will shew the Latitude and Longitude at Sea; serving also for weighing any Object, for measuring Space, or the Course of a Ship, and Time; shewing and keeping Account upon Dials, and upon cosmographical Columns, which are Part of such Machine; and also shewing the Lee-way of a Ship. Part of which Machine may also be applied to other useful Purposes. Dated December* 1807.—*Repertory of Arts, No.* 80, *Second Series.*

THE instrument by which all this is to be performed, its inventor denominates a *hydroscope*; and although it is not of a very complex nature, yet it is such as not to admit of a perspicuous description without the assistance of a plate. The following account, however, will probably convey a tolerably correct idea of both its principle of construction and its mode of operation. From the cabin, or any other convenient place of the ship, a cylindrical tube extends through her bottom into the sea; through this tube

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one or more globes, of about six inches in diameter, and of greater specific gravity than the water, are to be passed; and these are connected by a cord or flexible brass chain, passing over a grooved pulley at each end of the tube, with an horizontal spiral spring, which forms part of the hydroscope within. The resistance of the water on the globe causes the spring to draw out a small slide, which gives rise to a longitudinal and rectangular opening in the bottom of a kind of sand-glass, and permits the sand to descend into a scale attached to another spring fixed perpendicularly above it, and connected with a small hand which indicates, on a dial-plate fixed to the front of the machine, the weight of the sand run out in a certain time; and, consequently, the distance that the ship has run during the same period. The variation in the ship's rate or velocity is also pointed out on another dial-plate, on the side of the hydroscope.

When the instrument is used merely as a time-keeper, the sand will always run out at the same rate, and the time will be expressed regularly upon the side dial-plate: the box containing the sand is to be turned every twenty-four hours.

If this dial and its spiral spring be taken separately, they will constitute a convenient and ornamental weighing-machine to be used instead of scales. The same apparatus, in conjunction with the globe, forms an excellent perpetual log: and, by the addition of wheel-work, it will also serve to measure the strength of the wind.

The cosmographical columns, mentioned in the title to this specification, are one of them terrestrial and the other celestial; and are intended for the purpose of marking the daily situation of the ship with respect to the earth and the heavens: thus forming a kind of journal of the voyage.

An elastic cable, for the purpose of ascertaining the strength and direction of currents, also forms part of this patent right, as well as an instrument for determining the lee-way of a vessel. This last consists of a small vertical glass tube attached to the side of the hydroscope, and fixed into another horizontal tube which communicates with the water at both ends: and the lee-way is indicated on a scale by the rising of the water in the perpendicular tube.

Observations.—The advantages which will result from this invention, in the estimation of the patentee, are both numerous and considerable: and though we are by no means disposed to deny him the full merit of his ingenuity, which we sincerely hope will meet with its due reward, we do not think that this machine approaches so near to perfection as its inventor supposes. We trust, however, that the future efforts of General Grant's fertile invention will be successful in removing at least some of the objections to which his apparatus is at present exposed, and by that

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means in bringing his plan to such a state of maturity and usefulness as may render it justly entitled to the preference of all the machines that have yet been invented for the same purpose: and we shall feel a real satisfaction if any of our remarks should in anywise contribute to that end: while we assure him, should they fall under his notice, that they are made in a spirit which is the very opposite to that which would induce us to carp at a worthy individual, once high in rank in his native country, but now a foreigner in the land of his fathers.

In the construction of his hydroscope, General Grant seems to have assumed as a principle, that the resistances of fluids to bodies moving in them are in the ratio of their velocities; they are, however, *nearly* as the *squares* of the velocities. 2dly, We are by no means persuaded that the mode of measuring these velocities, by the discharge of fine sand through an aperture in the bottom of a box, is capable of affording an accurate result. From the nature of this sand, the lower particles will suffer a kind of hydrostatic pressure from those above them, and the quantity issuing through equal orifices in equal times will therefore evidently correspond in some degree (though this correspondence will probably not maintain any constant ratio) to the height of its surface *above* the orifice: whence it follows, that although this mode of measurement may answer very well for ascertaining *equal* portions of time under *similar* circumstances, it cannot be relied upon as an accurate measure of *unequal* portions of time under *variable* circumstances, for which purpose it is here adopted. 3dly, As the *standard* measure in this mode must necessarily be fixed by experiment, this undertaking will be attended with considerable difficulty before it can be ascertained with sufficient accuracy. If it be made with the log, the errors of that instrument will be incorporated with the result. If it be made in a river, where the velocity can be determined by actual measurement along its bank, the densities of river and sea water are not the same, and consequently their resistances will be different. 4thly, It is also asserted that the density of sea-water is different in different parts of the ocean, and more particularly in different seas; and therefore a true measure of resistance in one place will not be so in another. Our readers will perceive that these last two objections are the same in substance as were advanced against Mr. Boswell's invention for the same purpose, in our vol. iii. page 260.

The specification is accompanied with observations by the patentee, tending to shew the priority of his invention of this method of finding the longitude to the "method of ascertaining the rate of the velocity of a ship under sail," by Mr. Boswell, published in No. 61 of the Repertory of Arts, and above referred to. General Grant says, that his manuscript on this subject was deposited in the hands of Cadell and Davies, for publication, but

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he took it again for the purpose of submitting it to the Admiralty, the Board of Longitude, and the East India Company, during the years 1803, 1804, 1805, and 1806: that he had his work, on the means of finding the Longitude at Sea, printed in the beginning of 1807, by Mr. Shury, of Berwick Street, long before the appearance of Mr. Boswell's paper in the Repertory, which was in the No. for June of the same year; but it was not published till he had obtained his patent. He then adds, "It is hardly possible for a reviewer to give a more accurate sketch of a part of my work than has been done in that paper, except the addition of a stick which this writer proposes to pass through the powers I place in the water—an addition which can be of no use. To what source he is indebted for *his discovery* the public may judge, by comparing his account with the description I have given in my work of the instrument, long since communicated to men of science, to some of the official Boards, the result of thirty years of study, research, and experiment. *This writer* invites some unprejudiced commanders to attend *to the method*, and to publish the result of their experiments. I had myself communicated *this part* of my plan to *Lord Minto*, a short time previous to his departure for India, for the purpose of trying it on his passage."

Mr. Boswell has in the most unequivocal manner, in No. 76 of the work in which his paper was published, denied the charge brought against him by General Grant, and positively asserted that he had not even heard of General Grant's apparatus for finding the longitude, till some months after the publication of his paper.

We, however, who have strictly examined both methods, who are wholly unacquainted with either of these gentlemen, and are not conscious of any prejudice by which our judgment can be biassed, readily avow our persuasion that the charge of plagiarism has been brought upon insufficient grounds. The only similarity in the two methods is that of spring steel-yards being used for measuring resistance; but these were applied to this purpose upwards of a century since, and therefore ought neither to be claimed as an invention by either Mr. Boswell or General Grant, nor to furnish any ground of quarrel between two gentlemen, whose attentions have been so successfully directed to the improvement of nautical machinery. There is, therefore, nothing in either of these inventions which militates in the least against their having been made totally independent of each other, and which we believe to have been the case: and more especially so, as the contrary supposition must necessarily involve in it a breach of trust in some one of those respectable authorities to whom General Grant had submitted his method.

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Mr. RALPH DODD's *Patent for improved Bridge Floorings or Platforms, and fire-proof Floorings and Roofings, for extensive Dwelling-houses, Warehouses, and Mills. Dated June* 1808. —*Repertory of Arts, No.* 81, *Second Series.*

MR. Dodd's mode of fulfilling the title of this specification consists in the application of malleable iron and other metals, and condensed earth or artificial stone. The metal is made into tubical ribs and columns or shafts of the form required, and furnished with ears and flanges or joints, for the purpose of either fastening them to each other or any thing to them, as circumstances may require. The tubes may either be used empty or filled with condensed earth, or coated with the same as the user may think proper: and may be employed as rafters, joists, girders, piles, or stancheons, of any depth or breadth. The patentee also observes, that when made without ears they may be used as "masts, standards, yards, or booms."

Observations.—The purport of granting patents is to obtain the inventions of ingenious individuals, for the use of the community, by securing to them an exclusive right to the benefit of such inventions for a limited time, upon the condition that such inventions are described and illustrated in so clear and perspicuous a manner, that any common mechanic can easily apply them to practice. We fear this can hardly be said to be the case in the present instance.

Mr. CHARLES SEWARD's *Patent for Improvements in the Construction of Lamps. Dated November* 1808.—*Repertory of Arts, No.* 81, *Second Series.*

THIS is a variation of Argand's lamp. The reservoir of this lamp may be made of any form, provided it be as shallow as it conveniently may, in order that the oil may always be as near the flame as possible; and its size should be such as to contain only a sufficient quantity of oil for one time of burning. The wick tube is flat, and reaches quite to the bottom of the reservoir; by which means the oil receives a greater degree of warmth, and the wick is prevented from sliding down. The upper end of the tube is fixed to a round thin plate, which rests upon the top part of the reservoir, and one side of the bottom is cut away or turned up to make room for the wick. The plate and tube are kept in their proper positions by a screw, as in Miles's lamps.

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The lower part of the chimney or lamp-glass is globular, and the upper cylindrical. It is placed about half an inch above the plate which contains the wick tube, for the admission of air, and is supported by a strong wire, one end of which is fixed into the body of the lamp, and the other contains a spring or hoop, capable of embracing the upper part of the glass, while its lower end rests upon a flat support fixed to the wire. "These chimneys are not liable to be broken by the flame; they are also much stronger than cylindrical glasses, and have a handsome appearance, especially when the globular part is ground or roughened."

This description answers to the most useful and common form of these lamps, but the form may be varied while the "combination of improvement in the construction" remains the same. The improvements which the patentee claims as his, are, the additional length of the wick-tube; the contrivance at its lower end for giving room to the wick; the additional width of this tube; the shape of the lamp-glass; the manner of placing it; and the manner of supporting it. For each of these, as well as for the improved lamp itself, Mr. S. claims a patent right.

Observations.—We fully agree with this patentee when he says, in his observations at the end of this specification, that "light may justly be considered as one of the necessaries of life, and it is of so much importance, and composes so considerable an article of expense, that every improvement tending to facilitate its use, and economize its consumption, certainly merits the attention of the public." Several of these inventions, of which Mr. S. claims the merit, we are inclined to regard as real improvements; and we think that the construction of this lamp is altogether superior to that of any other with which we are acquainted. Of these improvements, his form of the chimneys, if it be justly entitled to novelty, is probably not the least. Glasses of the tubical form are very liable to be broken by the heat of the flame, particularly when a considerable light is required, and therefore add greatly to the expense of this kind of lamps; but by Mr. Seward's mode of constructing them, the glass is removed to a greater distance from the flame, and this defect in a great measure removed. The circumstance of their being made of roughened glass, we conceive to be a recommendation, and to be well calculated to remedy the prejudicial effects of a direct and glaring light upon the eye, which is the case when the chimneys are made of polished glass. Distinctness of vision depends as much upon simplicity in the shadows of objects as upon the intensity of light in the radiant body; and there are different degrees of intensity, which renders vision almost equally distinct and unfatiguing to the eyes. The qualities of a body which softens and

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disperses light, and those of another which intercept it, are very different, and of the former nature are those of roughened glass. Polished glass reflects a considerable part of the light; and a globe of this kind when surrounding a lamp is hardly seen, while one of roughened glass emits light from every part, which is thereby rendered more soft and pleasing to the eye, and very little of it is lost by internal reflection. And as a larger globe appears less luminous than a smaller one with the same degree of light; this effect may be regulated by the size of the globular part of the chimney. We apprehend, however, that the idea of using roughened glass for chimneys instead of that which is polished, did not originate with Mr. Seward; but that he probably derived it from some experiments of Count Rumford's on this subject, published in No. 55 of the Philosophical Journal, and noticed at page 151 of our vol. ii.

Mr. ZACHARIAH BARRATT's *Patent for a Machine for washing Linen and Cotton Cloths, and other similar Things; to which there may be affixed or omitted at Pleasure, a Contrivance for pressing the Water from them, now commonly done by Wringing. Dated October* 1808.—*Repertory of Arts, No.* 81, *Second Series.*

THIS machine consists of a wooden trough with an inclined bottom, and an uneven surface on the inside, made by grooves or projections. The projections may have pieces of leather or any other soft elastic substance fastened to them at short distances from each other, which are intended to act similarly to the fingers in the operation of washing. "These elastic pieces are not absolutely necessary, though the washing is performed better with than without them." A hole is made in the bottom of the trough to let off the water when done with. On the inside of the trough, near the top, and parallel to the ends, a roller is fixed, which is covered either wholly or in part with cork, leather, or some soft substance, to prevent noise when it is used. The operation is performed by a person pressing the cloths with a loose board, which is supported on the roller, and called an agitator. Its length is about two feet six inches, and its width nearly equal to that of the trough; the upper end of it contains holes or spaces for holding it, and the lower end is covered with leather or some other soft elastic material, with projecting pieces of some soft substance at the bottom, similar to those on the inside of the trough. A strong bar of wood is also fixed across the top of the tub, on which is placed the apparatus for pressing the water out of the washed cloths. This consists of a square box, with grooved

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or fluted sides and bottom for the readier discharge of the water, when the pressure is exerted by means of a piece of wood nearly equal in size to the interior of the box, acted upon by a vertical screw, supported by a frame. A lever, or any other means of causing a sufficient pressure, may likewise be used for the same purpose.

Observations.—We are by no means persuaded that this machine possesses any particular advantage over other machines that have been previously invented for the same purpose, in point of saving either trouble, or performing the operation more effectually. And we conceive that the business is never so well performed, and with so little injury to the cloths, as it is in the common way by the hand. Whatever may be the construction of a machine, we fear it can never be made to apply its effects to the different parts of the cloths subjected to its operation, in the proportion which is necessary for performing the business in that complete manner it is done by the natural and common mode.

Mr. Samuel Crackles's *Patent for a Method of making and manufacturing of Brushes from Whalebone, which have hitherto been made and manufactured from Bristles. Dated November* 1808.—*Repertory of Arts, No.* 81, *Second Series.*

The specification of this patent directs that the bone which comes from the mouth of the whale, be boiled or steeped in water until it become soft and flexible; in which state it is to be cut, with any suitable instrument, into pieces nearly resembling bristles, and of a greater or less degree of thickness or strength, according to the purpose for which the brushes are intended. The bone is then to be laid in a proper place until it be perfectly dry, when it is to be substituted in the room of bristles. "Those that are to be set with pitch may be seared or singed at one end with a hot iron, to make them resemble the roots; and beat at the other, to make them resemble the flag of the bristle."

Observations.—How much cheaper Mr. Crackles will be able to afford brushes of this manufacture than those which are made of bristles, we are not prepared to say, but we should apprehend they will be of a much inferior quality. Each bristle of which a brush is composed is a complete substance, and the continuity of its constituent particles is uninjured in the manufacture, which, particularly those of its cuticle, is of the most essential service in the preservation of its elasticity, upon which so much of the value and durability of the brush depends. In the mode of manufacture

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proposed by the patentee, the continuity of the particles of the whalebone is broken by the separation; the cuticle is completely destroyed; the lateral adhesion of the fibres rendered more easy to be overcome; and the comparative elasticity of each filament, and its consequent suitableness for the intended purpose, very much diminished.

Mr. EDWARD DAMPIER's *Patent for certain Machinery for rasping, grating, or reducing into small Parts or Powder, such Woods, Drugs, and other Substances, for the Use of Dyers and others, as are not easily to be pulverized by Percussion. Dated March* 1806.—*Repertory of Arts, No.* 82, *Second Series.*

THE machinery invented by Mr. Dampier, for the purpose set forth in this specification, consists of a large flat wheel, or other level surface, fixed on a vertical axis or arbor, and caused to revolve by the usual means. To the upper surface of this wheel, a number of cutters or raspers is fixed, with their faces toothed and directed upwards: each of these cutters is pointed with one end towards the centre of the wheel, either directly or with any inclination to the radii, so that the line of its length may every where cross the circles described by its teeth. The materials to be rasped are secured on the face of the said wheel, which by its rotation causes the rasps or cutters to act upon them, and the raspings fall through a perforation or long hole at the side of each rasp into a proper receptacle placed beneath the wheel. The wheel will admit of various forms, dimensions, bevelings, and velocities of motion; and the cutters may either be fixed on the wheel in connexion with each other or separately.

Observations.—The nature of this machinery is evidently very simple, and it appears to be well calculated to answer the purpose for which it was designed, by effecting the pulverization of hard and fibrous substances more completely, and with less trouble than any of the methods previously in use.

Mr. DAVID THOMAS's *Patent for a perforated Vessel, Percolator, and Frame, for making or preparing potable Coffee. Dated May* 1808.—*Repertory of Arts, No.* 82, *Second Series.*

THIS apparatus, as the title indicates, consists of three parts; the principal of which is an urn or vessel for receiving the beverage which is prepared from coffee by means of hot or boiling water made to pass through it, and which is drawn off by a cock adapted to the purpose. In order to prevent the filtration from

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being impeded or stopped by the compressed air in the vessel, a number of small perforations or flues are made in the upper part of the urn, to permit the rarefied air to escape. These are so situated, that when the percolation is finished they are covered with the same cover as the mouth of the urn, by which means the urn becomes a close vessel, from which neither the finer qualities or essence of the coffee, nor the heat, can escape by evaporation.

The percolator consists of a small box, which contains the coffee in its ground or pulverized state, and prevents it from rising and mingling with the water. This box is attached to the lower extremity of a cylindrical tube or vessel for receiving the water; its top and bottom are pierced throughout with very small holes, and the water being poured into the cylindrical tube above, passes through these and the whole mass of coffee into the urn.

The frame or stand is calculated to support the urn at a proper height for drawing off its contents, when it is not made of the common form. It serves also to enclose the vessel and its appendages, when they are made in the portable shape, and to protect them from injury.

In his observations the patentee gives a more particular description of this machine in its portable form, and calls it the coffee canteen, to distinguish it from other forms of the urn or receiver. It is of a cylindrical form, and separable into the following parts, viz. a receiver or urn, being a cylindrical vessel with a cover—a cylinder or barrel fitted to the inside of the urn—a percolator or charger fitted to the barrel—a case or stand for enclosing the whole, and supporting the canteen when in use. The office of the first three is the same as in the preceding description; and the frame is changed into a stand by depressing the upper end or cover, which is effected by means of four pins projecting from the edge of the cover, and sliding through perpendicular grooves, cut about half way down the inside of the cylindrical frame, at the termination of which they rest and form a stand for the urn at a convenient height.

The whole apparatus, including a caddy for ingredients, may be packed up in its cylindrical frame or case, which is six inches and a half in height, and four inches and a half in diameter.

Observations.—In this department of our labours, it is no uncommon circumstance to meet with a patentee who thinks much of a *little*, and makes use of a great number of high-sounding words and phrases, when a very few simple terms and expressions would convey his meaning with ten times as much clearness and precision. Mr. Thomas's opinion of his own invention, or at least the opinion which he wishes the public to have of it, may be ascertained with considerable accuracy from the two following sentences which conclude his observations: "And when it is

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considered that the whole apparatus, adapted to every use for which it can be required, and answering every purpose that could be aimed at, may be enclosed in a hollow cylinder, six inches and a half in height, and four inches and a half in diameter, it may safely be pronounced an unrivalled model of precision, utility, and elegance. To the curious it will not appear an uninteresting fact, that the construction and process, simple as they are made to appear, are founded upon pure philosophical principles; in which chemistry, pneumatics, and hydrostatics, have some share, and that without due regard to these, the machine would be ineffective." We, however, who regard things independent of inflated descriptions, having seen the apparatus, are ready to admit, that there are some advantages in this mode of preparing coffee over those previously in use. The compactness and portability of this apparatus will doubtless render it convenient and desirable on several occasions.

Mr. WILLIAM SHOTWELL's *Patent for certain Improvements in the manufacturing of Mustard. Dated June* 1808.—*Repertory of Arts, No.* 82, *Second Series.*

MUSTARD-BRAN (i. e. the hulls of the seeds) is to be wetted, then ground again, and afterwards mixed with a considerable quantity of water, which being well stirred, is left to settle, and, as soon as the coarser parts have subsided, the water is drawn off, and strained through a flannel or other strainer into a vat. In this vat the water is left to repose until the finer part of the bran has subsided, when the supernatant water is drawn off, and the residuary mustard preserved in the common mode.

The coarser part may be submitted again to a similar process; or the water containing the finer part may be used to wet a second parcel of bran.

In the processes, the same water should be used for several successive parcels of bran, the air kept from the mustard, and the operation performed as speedily as possible, in order to preserve the pungency.

To obtain dry flour of mustard from the bran, it may be ground with the *coles* of Indian corn, wheat-flour, white mustard-seed, or other appropriate substances; and thus a considerable degree of pungency may be obtained.

Mustard-flour may be expeditiously sifted, by fixing a hopper over the sieve frame, to supply the sieves, which sieves should be so hung that they may conveniently be brushed under the bottom, on long brushes may be fixed under the sieve, to be moved by machinery.

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Observations.—The first part of this patent, relating to the procuring of wet mustard, by re-grinding the hulls, and washing over the triturated mass, is certainly an ingenious mode of procuring as much eatable matter as possible from the brown mustard-seed; but the second part, in which the apparent quantity of dry flour of mustard is proposed to be increased by various additions, can only be regarded as an adulteration of this useful article.

That this adulteration is carried on to a very great extent is evident, because the manufactured article has not increased in price in proportion to the very great rise in the price of the seed. Gray, who introduced the manufacture in the beginning of the last century, had for a long time the monopoly not only at home, but also in Holland, and sold the flour pure, made, although not an expensive man, but a very moderate fortune.

The use of salt is perhaps the only adulteration, or rather addition, that seems perfectly allowable, because it is only a previous addition of that which would at any rate be subsequently added, and the pure flour will well bear twice its weight.

The original manufacturer was incommoded by the accumulation of his hulls, as they were then only used as a cosmetic wash to soften the hands; but the inelegancy of such a wash, and its cheapness, have, notwithstanding its superior efficacy, occasioned it to give place to more elegant and costly compounds. It was afterwards, and is still, mixed with 1-8th of ground pepper, and sold to the poor for pepper: to say nothing of its being used for the production of oil by expression.

AGRICULTURE.

Experiments on the comparative Culture of Wheat. By Robert Burrows, *Esq. of Great Witchingham, Norfolk.—Transactions of the Society of Arts, Vol. XXVI.*

The experiments detailed in this communication consist of two distinct courses; the first of which was made on clover stubble; the second on what is termed in Norfolk a bastard fallow, by which is meant a clover-ley ploughed up in July, and ploughed and harrowed two or three times between the first breaking-up and the time of sowing the corn. Both courses of experiments were made without any manure, under the impression that the quality could not be exactly proportioned, and therefore the crops were taken from land in previous good condition and very clean.

The modes of depositing the seed, which were brought into comparison in these trials, were the *broad-cast*, the *dibble*, and the *drill* husbandry; and the middle of a large field, without either tree or hedge-row of sufficient height to have any injurious

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effect, was selected for each course of experiments. The quantity taken was twelve acres, which was divided into three portions of four acres each, of which one extreme portion was dibbled, the middle portion sown broadcast, and the other extreme portion drilled with Cook's drill. The experiment *on the clover stubble* commenced on the 22d of September 1806, by ploughing the whole twelve acres nearly six inches deep, and laying it into stitches of ten yards in breadth, which were considered most convenient for the future operations: on the 25th the broad-cast was sown with two Winchester bushels per acre, and the seed immediately harrowed in by repeating the harrowings till it was completely covered, and then rolled with a heavy roller: at the same time the four acres on one side of the broad-cast were dibbled in equidistant rows of four inches and a half asunder, and the holes three inches apart from each other, which is the general method that obtains in Norfolk; from two to four grains were put in each hole, and the quantity of seed used was six pecks to the acre; and while the dibbling was going on, the other four acres were drilled with Cook's machine in equidistant rows of nine inches—a distance held preferable to all others in drilling, as, if the space be less, the horse-hoe can scarcely be used with safety, and without the use of the hoe all drilling must be imperfect. The wheat in each instance made its appearance in a few days after these operations were performed; the broad-cast first (which is attributed to its not being buried so deep in the earth as the other divisions); after the broad-cast, the dibbled came up, and last of all the drilled; and the latter, when all three were completely above ground, looked much more regular and beautiful than either of the others. The broad-cast seemed to have the advantage, in point of appearance, during the winter, and maintained it till the beginning of March, when the drilled began to assume the superiority, by exhibiting a fine dark green hue, with a broad reedy leaf, and maintained its advantage till the shooting of the ear destroyed all comparison of appearance. Next to the drilled, the broad-cast exceeded in health and promise, but lost its colour fast towards the end of May, and looked afterwards more sickly than the other two divisions, which is attributed to its being too thick to admit a proper circulation of air between the plants; and this circumstance is considered an evil always attendant on broad-cast crops.

The particulars of the expense of each of these divisions are accurately and minutely given, but do not admit of abridgment: in estimating the produce, the wheat is reckoned at 8*s.* per bushel, and the straw at 3*s.* per cwt. the average prices of Norwich market.

	£.	s.	d.
The *dibbled* wheat produced in value	80	10	0
The expenses were	10	17	3½
Profit	69	12	8½

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	£.	s.	d.
The *drilled* wheat produced in value	75	19	5
The expenses were	8	7	2
Profit	67	12	3
The *broad-cast* wheat produced in value	70	5	10
The expenses were	10	5	0
Profit	60	0	10

In these calculations, no account is taken of rent, tithes, or rates, or any interest on the capital employed; other tables, however, are added, in which these expenses are included, and the result is a profit of 8*l.* 8*s.* 11*d.* per acre on the dibbled, of 6*l.* 2*s.* 9*d.* per acre on the drilled, and of 5*l.* 19*s.* 6*d.* on the broad-cast crop.

The experiments on the bastard fallow commenced by ploughing up the clover-leys at the end of July, giving another ploughing the first week in September, a third the first week in October, and depositing the seed on the three last days of that month, and the first of November. The land was divided as in the preceding experiment, and the seed committed to the ground, as nearly as possible, in the same manner and quantity. The appearances, during the summer, were much the same as in the former instance; the drilled crop, from its first coming up, always looking the richest and most pleasing to the eye. In this experiment, the same prices are taken as in the preceding one, and

	£.	s.	d.
The *drilled* wheat produced in value	67	8	0
The expenses were	11	4	11½
Profit	56	3	3½
The *dibbled* wheat produced in value	68	8	10
The expenses were	14	6	3½
Profit	54	2	6½
The *broad-cast* wheat produced in value	61	17	11
The expenses were	12	4	6½
Profit	49	13	4½

These calculations, like the foregoing, are made without taking rent, rates, or profit of capital into the account; but subsequent tables are here also added, giving the net profit of 5*l.* 8*s.* 4*d.* per acre on the drilled; of 4*l.* 18*s.* 5¼*d.* per acre on the dibbled, and of 4*l.* 2*s.* 7¼*d.* per acre on the broadcast crop.

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It is stated, that these experiments were conducted with the greatest possible accuracy, and every operation performed under the eye of Mr. Burrows himself. He conceives the inferiority of all the crops on the bastard fallow to those on the clover stubble, though the soil was the same, a fine sandy loam, in both cases, and in his judgment in the same condition, to be a conclusive argument against fallowing land, which he calls "a barbarous system," and thinks it evident that the fallowist fallows away as much or more vegetable matter than an ample manuring will again restore to the soil: and, notwithstanding the superiority of the dibbled over the drilled in one course of these experiments, he retains his decided preference to the drill system, as affording the best means of keeping land free from foulness.

Observations.—After the extended analysis which has been given of this paper, it is unnecessary to praise the commendable accuracy with which the experiments have been conducted. It is one of the most valuable *records of facts* which has appeared, on the comparative culture of the corn which is the subject of it; we do not indeed recollect any previous experiments which have been so satisfactorily conducted. At the same time we cannot agree with Mr. Burrows in banishing fallows from all soils indiscriminately, though they may not be necessary on the light sandy loams of Norfolk: where land is too wet to admit of the drill, it can only be cleaned by resorting to a fallow.

Improvements in the Culture of Vegetables. By J. C. Curwen, *Esq. M. P. of Workington Hall.—Transactions of the Society of Arts, Vol. XXVI.*

The experiments which are detailed in this paper are considered by Mr. Curwen as discoveries highly beneficial to agriculture, and of the most important consequences, since they tend to establish the double advantage of well cleaning and working the ground; first, as it frees the land from weeds, and secondly, as it conduces to the growth of the crop. They are considered to afford a strong demonstration in favour of using manure in its freshest state, by which the expense of making dunghills will be saved, and the manure made to extend to the improvement of a third part more of the land.

The farm occupied by this gentleman was in such a state of foulness as to require a succession of fallows to clean it; but being unwilling to adopt a system attended with so much loss, he determined to attempt cleaning a part of it by green crops, and for that purpose to allow a greater distance between the rows than had ever been in practice. His first experiment on this plan was made on a crop of cabbages, which he planted in a quincunx form,

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allowing four feet and a half between each plant, that there might be room for the plough to work in all directions, as affording the greatest facility in cleaning the crop. The plants per acre were 2350, though 8000 is the usual number, and fourteen tons of manure were used instead of from thirty to forty, which was deposited as deep as the plough could penetrate, and the plants were set directly above it. A plough and harrow, so constructed as to work between the rows, were constantly employed during the summer, and the land kept as free from weeds as it could have been by a naked fallow. The produce of the crop was very great (thirty-five tons and a half per acre), many of the cabbages weighing 55 lb. each; though the quality of the soil was by no means good, and the plants were set very indifferent. The very rapid growth of the cabbages is attributed to the great evaporation from the earth, which takes place after every stirring, as is hinted by Bishop Watson, in his Essays on Chemistry. The success of this experiment induced Mr. Curwen to apply the same principle to potatoes, which were set in beds three feet long and two feet broad, leaving four feet and a half between each bed lengthways, and three feet endways; so that on an acre there were 1230 beds, each containing five sets, one at each corner, and one in the middle; and though the wetness of the season prevented any decisive conclusion as to this crop, yet it was quite as productive as other crops, and the land was kept perfectly clean. The cabbages were planted early in April, and the potatoes soon after.

It is stated, from experiments with glasses contrived for the purpose, that the evaporation from the ploughed land decreased rapidly after the first and second day, and ceased in five or six days, depending on the wind and sun; and the evaporation, after the most heavy rains, was not found to exceed what the earth afforded on being fresh turned up; and the evaporation from dung is stated to be five times as much as from earth, and to be equal to, on the surface of an acre, 5000 pounds per hour; and this is adduced as a strong argument for using fresh dung, before it is reduced by evaporation. It is also stated, that Mr. Arthur Young admits the great importance of evaporation, which he had hitherto overlooked, and that he is pursuing a course of experiments on the subject.

Observations.—This paper of Mr. Curwen merits more than ordinary attention, as it suggests a new principle by which vegetation may be promoted, and which applies equally to all soils and all situations. It might be deemed premature to pass our unqualified approbation on a plan which has been adopted by one individual only, but it appears both rational and philosophical, and we strenuously recommend further experiments to ascertain its utility.

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Improvement of waste Land. By J. BUTLER, *Esq. of Bramshot, Hants.—Transactions of the Society of Arts, Vol. XXVI.*

THIS gentleman purchased an estate at Bramshot, of which 70 acres were waste lands, growing a little timber, furze, and alder, and supporting a few cows in the summer, but never considered worth the expense of cultivation: twelve acres of this portion were the site of old fish-ponds, producing nothing but reeds and rubbish, eighteen acres produced a little sour grass, twenty-seven were quite a bog or morass, and seven grew very indifferent furze. The improvement was begun in 1804 with completely draining the land, by making an open ditch from five to seven feet deep in the lowest part of the bog, to let off the stagnant water; and other cross drains, which were afterwards laid with stones, were cut in various directions, to bring all the springs into the main drain. The expense of these drains was 338*l.* 2*s.* 11*d.* and 95*l.* more were expended in grubbing up the furze, and the stems of timber which had grown there. After the ground was become dry, and had been cleared of the rubbish and roots, the banks were levelled, and the holes filled up, and part of it laid on a slope for the purpose of irrigation, to which sixteen acres were subjected, and produced an excellent crop of hay, and good sheep-feed during the winter; part also was pared and burnt, and produced a good crop of oats, and some grew cole-seed for sheep. The whole is stated to be in a way of progressive improvement; and thirty acres laid down to grass are valued at 3*l.* per acre as meadow-land; 20 acres of reclaimed bog at 2*l.* per acre; and 15 acres of peat at 1*l.*; giving an annual value of 145*l.* though it was not worth five shillings an acre at the time of the purchase.

Observations.—The improvements described in this paper have been made with judgment, and (what we are glad to find) with much profit to the owner, but we do not see any useful discovery brought to light, or any unusual ingenuity displayed in improving this piece of common. The mode was obvious, and the prospect of advantage sufficient to induce any man of moderate agricultural knowledge to make the experiment.

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Account of planting an extensive Orchard at Bradwell in Essex. By Mr. SAMUEL CURTIS, *of Walworth.—Trans. Soc. Arts, Vol. XXVI.*

MR. Curtis converted a farm of nearly fifty acres, at Bradwell, into an orchard, by planting one hundred trees on each acre in rows two rods asunder, leaving a space of one rod between each tree in the rows; the plants are cherries and apples or pears alternately, so that one half of the plantation consists of cherry-trees, which may be cut out in twenty or thirty years when the apple-trees grow large and require more room, as they will then be left two rods asunder each way and in straight lines. In one part of the plantation medlars, quinces, plums, walnuts, and other trees are intermixed to make the collection complete. Farming crops have been since produced on the same ground as good as formerly, the plough being allowed to go within two feet of the trees each way, so that it is expected the land will pay the expenses for many years to come, and yield a profit exclusive of the fruit.

Observations.—Where fruit is as scarce as it is in Essex, plantations of the apple and pear must be very advantageous in point of profit, and the mode resorted to in this instance is well adapted for the purpose, as it preserves an income from the soil during the period the trees are coming to maturity; but perhaps it would be equally advantageous to use the orchard as a sheep-walk by laying it down to grass, which would promote the growth of the trees instead of purloining the nourishment, which corn crops necessarily must do, and consequently a less quantity of vegetable food will be left for the fruit-trees.

Method of curing the Foot-rot in Sheep. By Mr. RICHARD PARKINSON, *of Walworth, Surry. — Trans. Soc. Arts, Vol. XXVI.*

To effect this cure it is directed to pare the hoofs of the sheep affected with this disease, in such a manner as to leave no hollow to hold dirt; and if matter be formed, care should be taken to let it out, and the feet washed clean from dirt with stale urine, and wiped with a sponge; then the sheep are to be put into a house or shed, the floor of which has been previously spread about two inches thick with quick-lime reduced to powder by a small quantity of water; and the sheep should stand upon it for six or

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seven hours, when the cure will be effected, provided the lime be fresh from the kiln.

Observations.—The effect of this remedy is supported by two certificates, and the inventor was rewarded with the medal of the Society: the ease with which the experiment may be tried will recommend the application to the shepherd, whose flocks may be afflicted with this distressing malady.

A Method of stabbing hoven Cattle, to discharge the rarefied Air from the Stomach, when they have been over-fed with moist Clover-grass. By Mr. WILLIAM MASON, *of Goodrest Lodge, near Warwick.—Trans. Soc. Arts, Vol. XXVI.*

THE dangerous consequences arising from cattle being hoven are noticed; and the difficulty of one person applying the flexible tube is insisted on as a sufficient reason for introducing another remedy for a disease which might prove fatal before the assistance of a second person could be procured. For these reasons it is advised to use the trocar and canula, commonly used by surgeons, for the relief of hoven cattle, and to penetrate with the trocar and canula through the hide of the beast, to the paunch, on the near side, about six inches from the back-bone, at an equal distance from the last rib, and from the hip-bone; and then to withdraw the trocar and leave the canula in the wound, until the air has escaped from the paunch, when the canula may be taken out, and the wound covered with a plaster of common pitch spread on brown paper about the size of a crown piece. This operation is stated not to be injurious to feeding oxen, nor even dangerous to cows in-calf; and has been found particularly beneficial in preserving rearing calves, when afflicted with this disorder, which proves fatal to numbers of them.

Observations.—There can be no doubt entertained of the efficacy of this remedy, and it may be applied at all times with less risk than stabbing with a common knife, and with less delay than waiting for sufficient assistance to apply the flexible tube: the size of the trocar renders it also a convenient instrument to carry in the pocket at all times when cattle are feeding upon clover; and the operation can scarcely be performed in such a manner by the most ignorant servant, as to be attended with danger to the life of the animal.

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Refutation of Mr. Hunt's Absurdities. By Mr. B. THOMPSON, *of Redhill Lodge.—Agricultural Mag. No.* 18.

A comparative View of New Leicester and half-bred Merino Sheep. By Mr. JOHN WRIGHT, *of Pickworth.—Agricultural Mag. No.* 18.

Commercial Philosophy; or an Address to Mr. Robert Bakewell, of Wakefield. By Mr. JOHN HUNT.—*Agricultural Mag. No.* 18.

On the Imperfections and Inferiority of the Merino Sheep, and the Impropriety of introducing them into this Country, in Answer to Mr. Thompson. By Mr. JOHN HUNT.—*Agricultural Mag. No.* 19.

On the Spanish Cross. By a SUSSEX FARMER.—*Agricultural Mag. No.* 20.

On the Sublimity of agricultural Literature, or the Beauties of Thompson displayed. By Mr. JOHN HUNT.—*Agricultural Mag. No.* 20.

On Merino and English Sheep. By INDAGATOR.—*Agricultural Mag. No.* 20.

THESE controversial papers have originated from some positions laid down in a former number of the Agricultural Magazine, by Mr. Hunt, of Loughborough, who strenuously contends for the superiority of the New Leicester breed of sheep over the Merino race. They are all totally devoid of any useful information, but the deficiency is made up by superabundance of bitter personal abuse, and there are a few observations which might be advantageously transposed into an English "*Almanac des Gourmands.*"

On burning Clay for Manure, and on Merino Mutton. By Mr. FARBY, *of Crown Street, Westminster.—Agricultural Mag. No.* 18.

On burning Clay for Manure, in Answer to W. H. By CULTIVATOR MIDDLESEXIENSIS.—*Agricultural Mag. No.* 19.

THESE two papers owe their origin to an article in a former Magazine, noticing the clay found in Norfolk being frequently mixed with chalk, and suggesting that it might be advantageously used as manure when reduced to ashes by burning. Mr. Farey states, that the practice is found beneficial in the south of Lincolnshire, near Horncastle, where it obtains to a considerable extent; and the Middlesex Farmer gives his opinion in favour of the experiment, but thinks it must be vastly inferior to stable manure.

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On the State of Merino Improvement in Sherwood Forest, Notts. By Mr. CLAY, *of Blidworth.—Agricultural Mag. No.* 18.

IT is stated that there are about four thousand acres of land in the Forest of Sherwood, calculated only for the growth of fine wool, and that there are about the same number of sheep kept upon it, each fleece of which yields on an average at 49*s.* per tod of 28lb. about 4*s.* 4½*d.* The writer of this paper was induced to attempt improving his wool by crossing with a Spanish ram, and the result was, that each sheep cut more in quantity by a pound, and the quality at the wool-stapler's valuation was increased to 77*s.* per tod, thus increasing the value of each fleece to 9*s.* 7½*d.*; and this difference was obtained by one cross with a Merino ram and Forest ewes.

As some objections had been raised in that neighbourhood to the quality of Merino mutton, Mr. Clay represents it to be equal in appearance and taste to any mutton he ever saw, and strongly recommends the general improvement of the wool in Sherwood Forest by the introduction of Merino rams, instancing several noblemen and gentlemen who are furnishing examples of its beneficial effects.

Observations.—The recommendation of this writer is deserving of the closest attention on soils which are of the annual value of from half a crown to ten shillings an acre, where wool and not animal food is the principal object of the sheep farmer; and though Merino mutton may perhaps be superior in *quality*, an equal *quantity* cannot be obtained on rich land, as from Mr. Bakewell's Leicester breed.

Account of Summers's Turnip-chopper. By Mr. W. GOODMAN, *of Coventry.—Agricultural Mag. No.* 19.

On Summers's Turnip-chopper. By W. N.—*Agricultural Mag. No.* 20.

THE instrument described consists of three blades about six inches long, placed at about three inches distance from each other in a cross bar, and converging towards the end, for the purpose of cutting turnips into slices for cattle, after they have been drawn out of the ground: it was invented by Mr. Summers, of Tachbrooke, near Warwick, from whom it takes its name. In the first of these papers it is contended that this instrument is superior to all others for the purpose for which it is designed; but it is asserted in the second to be less valuable than a turnip-

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chopper of two blades crossing each other at right angles, so as to form a cross, which divides the turnip into four parts, and which is much used in Norfolk.

Observations.—Without pretending to decide a point which is of no real importance in itself, or to assign the palm of superiority in this case, we cannot but observe that the Norfolk turnip-chopper appears to us fully adequate to the designed purpose, and more simple in its construction than that of Mr. Summers.

On Double Cropping—Beans and Turnips. By TYRO.—*Agricultural Mag. No.* 19.

"THE mode of having these two crops together is pointed out to be by drilling the beans in every third furrow, and ploughing them in, which affords an excellent opportunity of cleaning the land, much more so than planting the beans in rows at 22 or 24 inches asunder: about the middle of April the hoeing commences, which should be done very deep and with great care; and the ground, by being first harrowed and then hoed, will be reduced to a fine tilth, and couch-grass and weeds may be readily picked out with forks. It is then ready for the turnips, the seed of which should be put in about the middle, and completed before the end of May, and no more sown in one day than can be covered over with the hoe, to secure the seed from the depredation of birds; and when the turnips get into broad leaf, they should be thinned by another hoeing, and the vacant places again sown with fresh seed. The beans should be separated at harvest either by reaping or pulling up by the roots. The advantages of this mode are represented to be very considerable, and the land to be in an improved state for barley or spring wheat.

Observations.—The practice noticed in this article has certainly novelty to recommend it, but we cannot implicitly subscribe to its utility from the instance here adduced. It may however be worth a trial on light land in a dry situation, where treading will not be productive of injury, but the expense attending it seems to preclude any idea of great advantage in a pecuniary view.

Description of an improved Water-cart. By W. N.—*Agricultural Mag. No.* 20.

"THE machine itself is nothing more than a large cask or barrel mounted on wheels and shafts in the usual way; but the improve-

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ment consists of the introduction of a small pump into the interior of the cask having the rod of the piston projecting through the top to be worked either by a lifting or a lever handle, and also having at its base a leather tube about twelve or fourteen feet long supplied at the end with a head composed of sheet iron, and perforated with holes for the admission of water, similar to that of a watering-pot, except being larger, and having the holes cut out instead of punched.

Observations.—The usefulness of this water-cart in those cases where the barrel is filled with a lifting pail from rivers, &c. must be obvious to every one, as it is calculated to save both time and labour, for it may be filled in a few minutes by a boy who could not lift a single pail; and it answers equally well with any other, where the barrel is filled from a pump. The simple and unexpensive addition to the common water-cart recommended in this paper is well deserving the attention of all who make much use of that machine either for agricultural or other purposes.

Thoughts on the best Rotation of Crops on a light Soil, where the Climate is damp and late. By G. I. D.—*Farmer's Mag. No.* 37.

The writer states that he occupies a farm, which is in general of a light and sharp soil, much intermixed with pebbles, and of a tolerably good quality, and his endeavour is never to crop it severely, lest the ground should become too tender, and the manure be wasted by being mixed with the substrata too rapidly. He usually lays lime the preceding year on the grass-sward which he intends to break up, which renders the ground free; and he gives it a single furrow in October, and sows it with wheat. The next crop is potatoes and turnips, and the third, barley or oats, with which grass-seeds are again sown, which are generally cut for two successive years, and then depastured for four years, after which they are again broke up, and the succession of crops is renewed: and his practice is to plough in oats and barley instead of sowing them in the usual way, which is represented to give to light land the advantages of a heavy soil.

Observations.—There can be no hesitation in recommending this paper as describing the valuable practice of a sensible and intelligent farmer, who is too prudent to sacrifice permanent gain to temporary advantage.

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Experiments made to determine the respective Increase made by Soiling and Grazing, commencing the 14th of May, and concluding the 1st Day of October 1808, *being a Period of* 140 *Days. By* J. C. Curwen, *Esq. of Workington.—Farmer's Mag. No.* 37.

The first experiment was made between two kyloes: one being ten or twelve years old, and having had a number of calves, was soiled; the other grazed was four years old, and had had no calves: the weight of the first at the commencement of the experiment was 66 stone; that of the other 57 stone. The increase in weight of each from time to time is stated in tables as well as the food consumed, and their respective weights when slaughtered; and the result is, that the soiled kylo gave a profit of 10½*d.* per day, and the grazed only 7¾*d.* per day.

The other experiment was made upon a long-horned cow, eight or nine years old, which had had several calves, and she was fed by soiling with a profit of 1*s.* 1*d.* per day. Some three-year-old Highlanders are also subjected to the experiment, and the result is expected to be equally favourable.

Observations.—The benefits resulting from soiling cattle either for the dairy or for slaughter are much greater than are generally believed, on account of the quantity of most valuable manure procured by this means; and the practice will become extended in proportion as the benefits derived from it are more generally known. The example of Mr. Curwen will be followed by many intelligent agriculturists. This article, though short, is highly deserving of perusal, *because it is a statement of facts given on the most indubitable evidence.*

On Horse-rakes for gathering Hay. By Osiris.—*Farmer's Mag. No.* 37.

After alluding to the circumstance of much hay being injured from the difficulty of obtaining sufficient hands to get it together in time, the writer recommends the use of a rake, or a combination of three rakes, to be drawn by a horse, as a substitute for raking hay-fields in the common way. He believes the form which would best answer the purpose to be that of the segment of a circle forty-five inches long, of cast metal, with wooden teeth sufficiently long, or the whole to be made of wood of sufficient weight; and he apprehends that two or three of these may be so

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arranged as to be drawn by a swivel-tree with considerable advantage and little inconvenience.

Observations.—Allowing much credit to the ingenuity of this writer, we cannot agree with him in the opinion that a horse-rake for hay is a desideratum in agriculture; for the drag-rake, which is used for corn in the midland and eastern counties, and which is about three feet long with teeth about eight inches long made of the points of old broken sickles, or of iron wrought into the same curve, is so well adapted for collecting hay, and so much more conveniently managed by the same-aged boy as would be requisite to drive the horse, that as much work may be done with it as with a horse-rake. Its advantages therefore are quite equal to those of the horse-rake, and the keep of the horse is saved.

On the Origin and Office of the Alburnum of Trees. By T. A. KNIGHT, *Esq.—Phil. Trans.* 1808, *Part II.*

THE letter commences by observing that the writer in a former communication had endeavoured to prove that the bark of trees is not subsequently transmuted into alburnum, in which case it is asserted that the experiments of Duhamel and succeeding naturalists afford sufficient evidence that the bark deposits the alburnous matter; for if the succulent shoot of a tree be examined at successive periods in the spring, it will be seen that the alburnum is deposited, and its tubes arranged in ridges beneath the vessels of the bark, and the number of these ridges at the base of each leaf will be found to correspond with the number of apertures through which the vessels pass from the leaf-stalk into the interior bark, the alburnous matter being deposited by a fluid which descends from the leaves, and subsequently secretes through the bark. Mr. Knight then, taking it for granted that the alburnum is deposited in this manner, proceeds to inquire into the origin and office of the alburnous tubes.

He observes that their position and direction have induced the greater number of naturalists to consider them as the passages through which the sap ascends; and indeed at their first formation when the surrounding substance is soft, they are filled with this fluid, to which they may either afford a passage upwards, or simply retain it as reservoirs till absorbed and carried off by the surrounding cellular substance; and though the former supposition is allowed to be the most probable at the first view, yet it is contended that the latter is more consistent with circumstances to be stated.

Of the different suppositions offered by naturalists to account for the ascent of the sap, two only are noticed, as appearing in

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any degree adequate to the effect; that of Saussure, jun. who supposes the tubes to contract as soon as they have received the sap, and by this contraction to force it upwards; and that which Mr. Knight himself communicated to the Royal Society in 1801, in which he suggested that the expansion and contraction of the compressed cellular substance, which contracts and expands with change of temperature after the tree has ceased to live, might produce similar effects, by occasioning a similar compression of the tubes. Both these hypotheses are however stated to be inconsistent with the facts now communicated.

The writer selected parts of the stems of young trees from which annual branches had sprung the preceding year; and by injecting coloured infusions into the stems through the annual shoots he ascertained that the tubes which descended from the latter were at their bases confined to that side of the stem from which they sprung, and to the external annual layer of wood; deep incisions were made in the stems of other trees, by which the communication through the alburnous tubes with the stem was destroyed, and yet the sap passed into the annual shoots in the succeeding spring; and though similar incisions were made in several lateral branches more than two lines deep in each on both sides in such a manner as that every alburnous tube was at least once intersected, yet the sap passed into these branches, and their buds unfolded in the succeeding spring, the incisions having been made in the winter; and on repeating the same experiment after the leaves were full grown in the summer, still the branches continued to live.

It is next remarked, that, though naturalists have agreed in stating that trees perspire most in the summer, and of course the sap must then ascend, yet that at this period the tubes of the alburnum appear dry, and to contain air only; it is conceived nevertheless to be evident that the sap cannot rise as a liquid through dry tubes, nor in any state through intersected tubes, and therefore it appears probable that it does not rise at all through the tubes of the alburnum, and that those tubes are intended to execute a different office. "For," says the writer, "if the sap does not rise through the tubes of the alburnum, it must rise through the cellular substance, and yet the passage of any fluid through this has been denied by almost every naturalist, probably because coloured infusions have not been observed to penetrate it, and because many naturalists have considered it as mere compressed medulla." He however contends that the fluid which generates the new bark exudes from it, for he had observed a new bark to be generated on the surface of the cellular membrane of pollard oaks; and if the sap in sufficient quantity to generate a new bark can pass through the cellular substance of an oak, it appears to him possible at least that the whole of the sap may ascend through it. He does not think that coloured infusions in any degree pass

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through the bark of trees, though it is evident that the sap readily passes through it; and therefore should it be proved that such infusions do not penetrate the cellular substance of the alburnum, the evidence arising from it would be very defective. Among other experiments to ascertain whether the cellular substance of the alburnum would imbibe coloured infusions, he took off in the summer, branches of two years old with the annual shoots and leaves attached to them, and effectually closed the alburnous tubes with calcined oyster-shells and cheese, which was covered with bees wax and turpentine to exclude moisture; a part of the bark was then taken off each branch in a circle round it, and the branches were placed in a decoction of logwood deep enough to cover the decorticated spaces; they were examined at the end of twenty hours, and the coloured infusion was found to have insinuated itself between the alburnous tubes in many instances apparently through the cellular substance; and this was most obvious in the walnut-tree, the young wood of which is very white. From these circumstances he is disposed to infer that the sap secretes through the cellular substance of the alburnum, and through this he conceives that it must ascend when the tubes were divided in a former experiment, and in those seasons of the year when the alburnous tubes are empty, though the sap must be rising with great rapidity; and besides, he conceives that the presence of the sap in the alburnous tubes during that part of the year, in which trees when wounded bleed abundantly, does not afford any evidence of the ascent of the sap through these tubes; for in the spring, when the buds of the sycamore first began to prepare for unfolding, he found that the sap abounded at the points of the annual branches, and at the same time flowed abundantly from incisions made into the alburnum near the root, yet when incisions were made eight or ten feet from the ground, no moisture appeared; and he also observed that the sap flowed as abundantly from the upper as from the under side of the incisions, so that it must have been by some means thrown into the tubes above the incisions, for the quantity discharged from them exceeded more than a hundred times that which the tubes could have contained at the time the incisions were made, even had every tube been filled to the extremity of the branch. And since it has been shewn that the sap can pass up when all the alburnous tubes are intersected, he thinks there is sufficient evidence that it must have been raised by some other agent than those tubes, and therefore ventures to conclude that the sap ascends through the cellular substance, for it does not appear to him difficult to conceive that this substance may give the impulse, with which the sap ascends in the spring; since if the cellular substance of the alburnum expand and contract, and be so organized as to permit the sap to escape more easily from one cell to another than in any other direction, it will be readily impelled to

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the extremity of the branches. If the sap be raised in this manner, it is remarked that much of it will probably accumulate in the alburnum in the spring, because the powers of vegetable life are the most active at that season, and the leaves are not enough advanced to discharge it by perspiration; the cellular substance, being then overcharged, may force a part of its contents into the alburnous tubes, which thus become reservoirs, and are filled to a greater or less height in proportion to the excess; and if the tubes thus filled be divided, the sap will flow out of them, and the tree will be said to bleed, till the leaves are unfolded and execute their office of affording perspiration, by which the sap will be drawn from its reservoirs, and the tree will cease to bleed at any wounds made in it.

The alburnous tubes also appear to this writer to answer another purpose, and to be so in some degree analogous to the bones of animals, by which the necessary degree of strength is given with less expense of materials or the incumbrance of unnecessary weight; and to this it is attributed that many woods are at the same time very strong and very light, for in all which possess this property the rigid vegetable fibres are placed at greater distances from each other by the intervention of alburnous tubes, and consequently act with greater mechanical advantage than if placed immediately in contact.

Taking notice of his having shewn in a previous communication that the specific gravity of the sap increases during its ascent in the spring, and that saccharine matter is then generated which had no previous existence either in the sap or the alburnum, Mr. Knight conceives it probable that the air contained in the alburnous tubes may assist in the generation of this saccharine matter, for he has found that much air is absorbed during the process of grinding apples for cyder, and that during this absorption of air the juice of acid apples becomes sweet, and acquires an increased specific gravity; and he states that a similar absorption of air with corresponding effects is known to take place in the process of making malt. And though he retracts an opinion he once entertained respecting the ascent of the sap in the alburnous tubes, he does not alter his former sentiments respecting the subsequent motion of the sap through the central vessels, leaves, and bark, or the subsequent junction of the descending with the ascending current in the alburnum, since all his experiments tend to confirm those conclusions.

Observations.—Mr. Knight holds perhaps the first rank among those gentlemen who have carried their researches into the economy of vegetation, and every thing that falls from him on that subject is deserving of very great attention. This paper may be considered rather as a continuation of former communications to the Royal Society, than as a detached article, and will be best

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understood by a reference to his former papers. He has however evinced beyond a doubt that the alburnum of trees does not raise the sap through its tubes; but if the sap ascends, as he supposes, in the intervals between them, the difference is immaterial as to the effects on vegetable life, and the bleeding of trees above any incision made in them is philosophically and satisfactorily accounted for.

Description of a Gauge, or Measure, for Timber. By Mr. James Broad, *of Downing Street, Westminster.—Trans. Soc. Arts, Vol. XXV.*

This instrument (the usefulness of which is very copiously descanted upon by the inventor) is composed of two straight pieces of deal about thirteen feet long, made rather taper, which are joined together by a pin near the larger end, on which they are moveable; and at a little distance from the same end is a brass limb or index, graduated in such a manner as to shew the quarter-girth of the tree to which the forceps part is applied. To take the height of a tree it is recommended to use deal rods of seven feet long, made so as to fit into ferrules at the end of each other, tapering in the same manner as a fishing-rod, with the feet marked upon them, as a set of five would enable a person to measure a tree of more than forty feet high with great expedition and convenience, as he would be able to reach himself about seven feet from the ground.

Observations.—If the invention of these forceps for taking the girth, and the recommendation of seven-feet rods for ascertaining the height of timber, are to be estimated by their utility to buyers and sellers of standing timber, Mr. Broad merits the thanks of that class of the community for the ready and simple method of discovering the value of the subject to be disposed of. The *utility* of the invention was deservedly rewarded by the silver medal of the Society.

On Embankments. By Lord Boringdon, *of Saltram, near Plymouth.—Comm. Board of Agric. Vol. VI. Part I.*

His Lordship informs the Board that in 1806 he constructed an embankment across some lands adjoining his park near Plymouth, by which he reclaimed from the sea 175 acres, which were previously of no value from being overflowed by the tide; the soil is rich argillaceous loam mixed with sand and shells, and strongly impregnated with the marine acid; the embankment was

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raised 16 feet, and the back slope, next the sea, which was covered with lime-stone paving, was made four times the height, and the inner slope, sown with grass-seeds, twice the height. Part of the newly-acquired land is cultivated, and the rest bringing into cultivation; the only preparation previous to ploughing was covering the ground for some time with fresh water, for the purpose of extracting the marine acid. The expense incurred was between nine and ten thousand pounds, and the value of the land reclaimed is estimated at about twenty-seven or twenty-eight thousand.

Observations.—The improvements on the shores of the Lary branch of Plymouth harbour certainly add to the beauty and value of Lord Boringdon's park, but we apprehend he will not find many agriculturists reclaim land from the sea at the very great expense of 60*l.* an acre.

Enclosure of Marshes from the Sea. By Admiral BENTINCK.—*Communications to the Board of Agriculture, Vol. VI. Part I.*

THE Admiral possessed a salt-marsh of 180 acres of land, which produced about 30*l.* a year; this he embanked from the sea in 1807, by raising a bank twelve feet high on a base of 72 feet, so divided as to give a slope of four feet to every foot of height on the sea side, and two feet to each foot on the inside. The length of the bank was rather more than a mile and a half, and the expense of enclosing about 4000*l.* The land is stated to be let on a seven years lease at four guineas per acre.

Observations.—This land being reclaimed from the sea, at an expense of not more than twenty-five pounds per acre, and being let for four guineas annually, affords much encouragement to attempt similar improvements; which is not the case with Lord Boringdon's embankment, noticed in the preceding article, which was made at more than double the expense.

On Fences. By the Rev. JAMES WILLIS, *of Sopley, Hants.*—*Communications to the Board of Agriculture, Vol. VI. Part I.*

THIS paper, which is of considerable length, is mostly occupied by arguments in favour of enclosures, and statements of difficulties in carrying them into effect, which have been noticed a hundred times before; an account is also given of the method pursued by a Mr. Sleat, who appears to be a spirited agriculturist, in reclaiming a new enclosure, part of the commons of Christ Church, Hants; but the only part which relates to the title is a

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description of the bank fences made by that gentleman; they were made six feet wide at the bottom, two feet wide at the top, and six feet high, and the top was planted with willow-stakes crossing each other like net-work; the tops of the banks were left concave, that moisture might be retained to promote the growth of the willows, it having been found, by previous trials, that they did not succeed equally well on banks which were rounded at the top: the banks were faced on both sides with the clods of earth dug on the spot, and the rubbish earth was put into the centre or filling of the bank; and those willow truncheons were found to grow the best which were laid in during the making of the bank, as in that case the bark was not injured.

Observations on the Influence of Soil and Climate upon Wool (under the Title of Commercial Philosophy), addressed to Mr. Bakewell, of Wakefield. By Mr. JOHN HUNT, *of Loughborough.—Agricultural Magazine, No.* 15.

On the new Leicester and Merino Sheep, in Answer to Mr. Hunt. By CULTIVATOR MIDDLESEXIENSIS.*—Agricultural Magazine, No.* 15.

On Merino, new Leicester Sheep, &c. in Answer to Mr. Hunt; By Mr. BENJAMIN THOMPSON, *of Redhill Lodge, Notts.—Agricultural Magazine, No.* 16.

On Wool; Reply to Mr. Hunt. By Mr. BAKEWELL, *of Wakefield.—Agricultural Magazine, No.* 16.

Observations on the Influence of Soil and Climate upon Wool (under the Title of Commercial Philosophy, Letter 2d), addressed to Mr. Bakewell, of Wakefield. By Mr. JOHN HUNT, *of Loughborough.—Agricultural Magazine, No.* 16.

On the Merino Question: the Critic unmasked, or Truth without Disguise, in Answer to Cultivator Middlesexiensis. By Mr. HUNT, *of Loughborough.—Agricultural Magazine, No.* 17.

THESE six papers are of very considerable length, but are purely controversial, and have originated in an article by Mr. Hunt, "on the Perfections and Superiority of the Leicestershire Breed of Sheep," of which we gave an account in our last volume, p. 261. They are merely noticed here for the information of such readers as choose to pursue the controversy; and our opinions may be collected from the observations on Mr. Hunt's original article.

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Contents of England, Wales, and Scotland, in Acres, calculated by Mr. CARY.—*Communications to the Board of Agriculture, Vol. VI. Part I.*

THIS article specifies the number of acres in each county of Great Britain, making together a total of 55,193,060 acres; of which England contains 31,929,340; Wales, 4,320,000; and Scotland, 18,943,720.

Economical Dwellings for small Proprietors of Land. By ARTHUR YOUNG, *Esq. Secretary.—Communications to the Board of Agriculture, Vol. VI. Part I.*

MR. Young observes, that in a country where taxes are much on the increase, the *dwelling* ideas of gentlemen should be reduced, or distress must be the consequence; and the communication is accompanied with a plan for a dwelling, which combines the gentleman's house with the farm house, on a scale "which," he says, "will not be burdensome to a gentleman with a clear 800*l.* per annum, and a prudent man with 2000*l.* will not regret the want of a better." The plan is a very excellent one, and merits the attention of those for whose use it is more particularly designed.

CHEMISTRY AND MINERALOGY.

Method of painting Linen Cloth in Oil Colours, to be more pliant, durable, and longer impervious to Water, than in the usual Mode. By Mr. WILLIAM ANDERSON.—*Trans. Soc. Arts, Vol. XXVI.*

THE paint usually laid upon canvas hardens to such a degree as to crack, and eventually to break the canvas, which renders it unserviceable in a short time: but the canvas painted in the new manner is so superior, that the Navy Board has ordered all canvas used in the navy to be thus prepared; nor is this the only advantage, for a saving of one guinea is made in every 100 square yards of canvas so painted.

The old mode of painting canvas in the King's yards was to wet the canvas, and prime it with Spanish brown, then to give it a second coat of a chocolate colour made by mixing Spanish brown and black paint, and lastly to finish it with black.

The new method is to grind 96 lb. of English ochre with boiled

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oil, and add 16lb. of black paint; which mixture forms an indifferent black. A pound of yellow soap dissolved in six pints of water over the fire is mixed, while hot, with the paint. This composition is then laid upon the canvas (without its being wetted as in the usual way) as stiff as can conveniently be done with the brush, so as to form a smooth surface. The next day, or still better on the second day, a second coat of ochre and black without any or but a very small proportion of soap is laid on; and allowing this coat an intermediate day for drying, the canvas is finished with black paint as usual, three days being then allowed for it to dry and harden; it does not stick together when taken down and folded together in cloths containing 60 or 70 yards. Canvas finished entirely with the composition, leaving it to dry one day between each coat, will not stick together, if laid in quantities.

From actual trials of near four years, the solution of yellow soap is a preservative to red, yellow, and black paints, when ground in oil and put into casks; as they acquired no improper hardness, and dried in a remarkable manner when laid on with the brush, without the use of the usual drying articles.

Immense quantities of unserviceable ships' hammock cloths have been destroyed at the King's yards in order to prevent embezzlement; but means have now been found to obtain the colour laid on them at a small expense, which is effected by burning this unserviceable canvas, taking care to rake aside the ashes and sprinkle them with water, in order to prevent too great a degree of calcination. The ashes are then prepared for grinding, by being sifted. Thus 4 cwt. of dry colour valued at 9*l.* 6*s.* have, at the expense of 6*s.* been obtained from a ton of canvas. Canvas painted with black paint only, produces a black colour; if painted black upon a yellow or red ground, it produces a dark chocolate; if painted lead colour, it produces a dark lead.

A great quantity of paint being required at Portsmouth yard, and the allowed number of hands not being sufficient to grind it in the common mills, induced a trial of employing one man to grind with four mills at a time, by placing two mills on each side of the winch. A space of two feet six inches is allowed between the two pairs of mills, for the man to stand between them; and the mills of each pair are placed so close as only to leave room for a fly wheel to play between them. Fly wheels at the extremity of the axis are impediments.

Observations.—We have no doubt but that these processes are considerable improvements. As soap is so well known to be miscible with oily substances, it is surprising that it or the alkali of which it is composed, have not yet been brought into use in the composition of oil colours, either for immediate consumption, or for painting stores.

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We regret that the moral state of sea-port towns, as being places in which the mercantile spirit has its full influence, is such as to render it necessary to destroy the unserviceable articles in the naval storehouses; but since we feel this necessity, it must be allowed to be some improvement to make the most of the products of their combustion. Mr. Anderson has therefore done an acceptable service to the nation by proving experimentally the practicability of using those products in painting. His caution of moderating the combustion, by raking away the half-burned canvas and extinguishing it with water, is essentially necessary to be attended to, as otherwise, if the combustion were completed, a very small quantity of a white saline ash of no use in painting would be yielded by the canvas itself.

Method of preserving Fruit without Sugar, for House Use or Sea Stores. By Mr. THOMAS SADDINGTON.—*Trans. Soc. Arts, Vol. XXVI.*

THE expense of sugar is frequently urged as a reason for not preserving our English fruits, and to this may be added the uncertainty of success from the strong fermentable quality of many fruits. They may indeed be preserved for a length of time without sugar, by baking or boiling, and then closely stopping them up; but if the cork becomes dry, the atmospheric air exchanges place with what is impregnated by the fruit, which then soon becomes mouldy: but fruits may be preserved in good condition, by the following process, for two years, or even it is probable for a longer period even in hot climates, as some that were done in 1800 have been exposed in an upper room to the sun during the whole of the summer without injury.

The fruit being clean picked, and not too ripe, is to be put into wine or porter bottles, as they are cheaper than what are called gooseberry bottles, and can be procured in places where the latter cannot be had. The bottles must be filled as full as they can be packed, and corks being stuck lightly into them, they are to be placed upright in a kettle of water and heated gradually to about 160 or 170° Fahr.; that is to say, until the water feels very hot to the finger, but does not scald. This degree of heat is to be kept up for half an hour, and then the bottles being taken out one by one, they are to be filled up to within an inch of the cork with boiling water, the cork fitted very close and tight, and the bottle laid on its side, that the cork may be kept moist. To prevent fermentation and mould, the bottles are to be turned once or twice a week for the first month or two, and once or twice a month afterwards.

When applied to use, some of the liquor first poured off serves to put into the pies, &c. instead of water, and the remainder being boiled up with a little sugar will make a rich and agreeable syrup.

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The fruit ought not to be cracked by the heat; some trials were made by keeping the bottles in a heat of 190° for three quarters of an hour, but the fruit was reduced nearly to a pulp.

Although wine or porter bottles have such narrow necks, the fruit may be picked out with a bent wire, or iron skewer.

Samples of apricots, gooseberries, currants, raspberries, cherries, plums, Orleans plums, egg plums, damsons, Siberian crabs, green gages, and rhubarb, have been presented to the Society.

In the summer of 1807, it cost about 1*l.* 9*s.* exclusive of the charge of the bottles and corks, to preserve 95 bottles of fruit; which were worth in the winter one shilling each, so that there was a clear gain of 200 per cent.

Observations.—That fruit, if not too ripe, might be preserved by the bottles in which it was packed being heated in a water bath, and then closely stopped, has been long known; but so many minute circumstances were necessary to insure success, that the addition of boiling water must be esteemed a great improvement, and we assure Mr. Saddington that we ourselves mean to put his process into execution this summer, and have not the least doubt but that our families and guests will frequently give him their thanks in the ensuing winter.

It is expressly said that the bottles should be filled with water to within an inch of the cork: but would it not be better to fill them entirely with water? for what good purpose can be answered by leaving any bubble of air within the bottle, which renders it further necessary to turn the bottle occasionally?

It appears to us also that the water with which the bottles are filled, ought to have boiled strongly for some time, in order to disengage it as much as possible of the air it contained.

Apparatus for making carbonated Hydrogen Gas from Pit Coal, for the Purpose of lighting Factories. By Mr. Samuel Clegg. *—Trans. Soc. Arts, Vol. XXVI.*

The coals are introduced into a cast iron cylinder placed on its side over the fire, by the circular base which is even with the external surface of the furnace, closed by an air-tight iron plate, which is fastened by a screw in the centre. The inferior side of this cylinder is protected from the fire over which it is enclosed in brick work, by a larger half-cylinder of cast iron, placed at some distance below it, by which means the heat is more equally distributed to the pit coal.

A cast iron pipe proceeds from the upper side of this cylinder to a cast iron receiver, which is situated at the bottom of the well in which the gazometer rises and falls: in this receiver the

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tar and other condensible products are collected, and are extracted from time to time by means of a pump fixed to it.

From the top of this receiver proceeds another iron pipe, which reaches to the surface of the water in the well, but which is inserted into an air-holder of about 18 inches in diameter and two feet long made of iron. The lower part of this air-holder is pierced with several small holes, and thus it serves a double purpose, first to divide the gas into several small streams, and thus to render it purer by washing it as it passes through the water; and secondly it serves as a reservoir of gas, from whence the tar receiver, connecting tubes, and even the retort itself, may be filled with gas, whenever an absorption takes place, by the retort being cooled or otherwise.

The gas is discharged from this air-holder into the gazometer, which is suspended over the well, and rises and falls therein, being balanced by two weights passing over pullies. This gazometer is made of wrought iron plates, luted in the seams, so as to be air-tight, and well painted both within and without; it has an iron pipe made fast in the centre by means of two sets of stays, one at the bottom of the gazometer and the other at the top. An upright pipe fixed in the centre of the well passes up this central pipe of the gazometer when it is depressed in the well. The gas is pressed out of the gazometer through a row of holes at the very top of the central pipe (so that only the transparent gas is used) into that pipe, whence it passes into the centre pipe of the well, which is continued across the well and up the side, and from thence is branched out to the lamps.

Each of these lamps will consume about six cubic feet of gas in an hour. They are composed of two concentric tubes closed at bottom by an annular plate; the gas is introduced between them by a stop-cock on the side, and emitted for combustion by a row of holes in the annular plate which connects the two tubes at the top. To assist the combustion, the current of air that passes through the inner tube is directed against the flame by a button at the end of a wire, which slides up and down the inner tube, and thus the button can be placed at any required distance from the upper orifice of the tube. This current is also determined to the flame, by the upper orifice of the inner tube being enlarged; a glass chimney is also used, which is supported on an annular plate pierced with a row of holes to admit the air to the external surface of the flame.

A gazometer to contain 700 cubic feet of gas will weigh about 20 cwt. and cost about 2*l.* 10*s.* per cwt.

A complete apparatus for 40 lamps, each equal in light to 10 candles of eight to the pound, and to burn for four hours, will cost about 250*l.*

Observations.—This appears to be an ingenious and well-constructed apparatus, and may probably be used with much advan-

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tage in lighting up large manufactories. It can only be ascertained whether it will admit of general economical application when we shall have accurate comparative statements.

On the Purification of Camphor by means of Potash. By PHILOCHEMICUS.—*Phil. Journ. No. 97.*

IT was conceived that pure potash has a stronger affinity for the fixed, than for the essential oils; and camphor being considered as one of the latter in a concrete state, it seemed probable that the alkali might be used with effect for its purification.

Several experiments were made with very impure camphor (not the unrefined camphor of commerce), mixed with different fixed oils, sand, and a minute quantity of potash; the camphor being sublimed, the oily particles and the empyreumatic odour remained with the alkali. The subcarbonate does not answer the purpose, as its affinity for oils is less than that of pure potash.

The heat in one case was more than sufficient, yet the camphor produced was free from empyreumatic odour and a little whiter than usual.

The proportions then used were,

6 parts impure camphor.
3 olive oil.
44 sand.
1 potash.

The product was the same when linseed oil was substituted.

Observations.—From the analogy between potash and lime, it might be expected that the former, as well as the latter, might be used with effect for the purification of camphor.

Accurate comparative experiments can alone determine whether this mode will be most economical.

Account of a simple and economical Method of preparing an artificial Cheltenham Water highly impregnated with carbonic Acid. By RICHARD GREENE, *Esq. of Cork, A. B. Trin. Col. Dub. M. D. and President of the Royal Medical Society, Edinburgh.—Phil. Journ. No. 97.*

THE natural Cheltenham waters have been long and deservedly celebrated for their medical uses. Their virtues are chiefly owing to the sulphates of soda and magnesia: They contain also about one eighth in bulk of carbonic acid, which, in one gallon of the water, holds in solution nearly five grains of iron. The sulphate.

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and carbonate of lime which exist in them seem to have no effect on their medicinal properties.

The carbonic acid used in making artificial mineral waters is procured by treating any of the natural carbonates of lime, as chalk, marble, &c. with diluted sulphuric acid. The gas is then by mechanical pressure forced into the water.

The carbonate of soda and magnesia contain a large quantity of carbonic acid; the former about one sixth, and the latter nearly half its weight. They are decomposed by sulphuric acid, and the salts of Cheltenham water are formed.

It occurred to Dr. Greene, that if a sufficient quantity of water were present, and the gas, during its disengagement, could be confined, the effect would be similar to mechanical compression. Many experiments were made on this idea with perfect success.

The volume of carbonic acid which might be confined with safety in a half-pint bottle, was ascertained, and the quantities of the carbonates and diluted sulphuric acid required being known by calculation, the carbonates were put into the bottle with one grain of sulphate of iron, and filled with cold water, except a small space for the sulphuric acid, which being introduced, the bottle was corked and secured with a string. By a little agitation the salts soon dissolved, and the liquor became transparent. The string being untied, the cork was driven out with considerable violence, and the water scarcely differed in taste from the best soda-water.

The materials used in this experiment were nearly one half greater than Dr. Greene recommends, which shews that the strength of the water may be much increased without bursting the bottles.

The carbonate of soda employed was the common salt of commerce, prepared from muriate of soda by the patent process; it neither deliquesced nor effloresced by exposure to the air, and gave no indication of lead, which it has been supposed to contain, when made in this way.

He prepared the carbonate of magnesia from sea-water, and could depend on its purity.

The sulphuric acid was the diluted acid of the Edinburgh college, of the spec. grav. 1.0735, and was made by adding one part of the acid of commerce, of the spec. grav. 1.852, to eight parts of water.

The following are said to be the best proportions of these substances to half a pint of water:

Carbonate of soda - - -	40 grains troy.
——— of magnesia - - -	20 grains ditto.
Sulphuric acid - - - -	250 grains by measure, or a quantity equal in bulks to 250 grains of water.

In this case there is a slight deficiency of sulphuric acid, which should always be observed, for the excess of carbonic acid will

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dissolve any carbonate of magnesia that may remain; but an excess of sulphuric acid gives the water a disagreeable taste, and might injure the teeth, and affect the bowels, if irritable.

These quantities produce about 35 cubic inches of gas, which in a half-pint bottle is equal to the additional pressure of an atmosphere and half on the sides of the bottle, or of two atmospheres and half on the surface of the water.

The sulphuric acid employed should be diluted: in this case, if any lead be present, it will be precipitated.

The quantities here specified may not always exactly answer, as the carbonate of soda is not uniform in its composition, nor the acid always of the same strength; any excess, however, will be apparent, by an acid taste in the water after it has been exposed to the air for some time; and any considerable deficiency, by a part of the magnesia remaining undissolved. In either case the acid must be increased or diminished as indicated.

To ascertain the exact quantity of acid necessary to a given weight of the carbonates, add about an ounce of water in a wine-glass, introduce the acid gradually, and stir the mixture till they are dissolved, and no effervescence appears, taking care that the acid is not used in excess, which is known by its changing infusion of litmus or red cabbage-leaves to red. Note exactly the quantity of acid used, and make a measure containing ten grains less, which is easily done by a glass tube and a file. If this succeed well, a quantity of the same may be procured and kept in close vessels.

The bottles to be used should be previously furnished with corks, that no time may be lost after the acid is introduced; and they should remain on their sides, that no gas may escape. The magnesia should be in fine powder, to dissolve more readily. The soda should not be so, as by its slow solution the magnesia will meet the acid in a more active state. By observing these precautions, a bottle of the water may be made ready for use in less than a minute. It will keep well: shake the bottle before it is opened, as the iron precipitates in part.

The strength of the water may be easily varied by altering the quantities of the materials, observing the relative proportion of the acid to the carbonates.

For a pint bottle, the quantities are,

Carbonate of soda	80 grains.
——— of magnesia	40 ditto.
Sulphuret of iron	2 ditto.

This is a full dose for an adult, and more than should be taken for a constancy during a course of this water. The expense of preparing a pint bottle is only about one penny.

Observations.—This appears to be a simple and economical method of obtaining artificial Cheltenham water, and it will probably be connected with general utility.

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We have some doubts whether this idea is original, though we by no means wish to detract from the merits of Dr. Greene, to whose experiments the public will be much indebted for a ready and a cheap supply of this water.

Second Letter on the Advantages of Coal Gas Light. By Mr. B. Cook.—*Phil. Journ. No.* 98.

When no soldering is required in the construction of the apparatus, it will be much less expensive, and the saving is nearly the same. Mr. Cook has found, that one lamp consumes nearly as much gas as twelve lights or candles; hence less gas is required, and it will be sufficient to make it two or three times a week. The process of making the gas is so simple and easy, nothing more being necessary than filling the retort with coal, and keeping a fire round it, that a boy, or the master of the shop, may attend to it without neglecting other business.

Mr. Cook recommends the use of the patent lead pipes of three quarters of an inch in diameter, in preference to old gun-barrels. The advantage attending gun-barrels did not answer his expectation; being in short lengths, the soldering places were many, and if they are not quite air-tight, the gas escapes, and a disagreeable smell is produced. Lead pipes may be easily bent, and as they have fewer joinings, they are made air-tight with less difficulty. They are easily removed, and when worn out, may be sold for a great part of their original cost.

The following statement (when lights are only used twenty weeks in the year) may be considered as generally true:

Dr.	£.	s.	d.	*Cr.*	£.	s.	d.
Coals for 20 weeks, at 1*s.* 3*d.*	1	5	0	Twenty weeks, at 18*s.*	18	0	0
Interest on 30*l.* or 25*l.* the cost of the apparatus	1	5	0	Coak	0	12	6
Saved	16	2	6				
	18	12	6		18	12	6

Mr. Cook has only calculated on the saving of twenty candles; more may be lighted with only the additional expense of a little more coals. If 25 or 30 candles were used, the advantage would be greater. In the above statement, however, it is sufficiently demonstrated.

Mr. Cook states, in reference to an objection, that the smell occasioned by the gas, far from being injurious to the health,

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rather tends to preserve it, by destroying contagion and purifying the air, and he attributes the prevention of diseases in our large manufacturing towns to the smoke of coals.

Observations.—Information of this kind has long been wanted, and those who have made the greatest bustle on the wonderful advantages attending the use of the gas lights, have in this respect been most deficient. The statement of Mr. Cook clearly proves the great advantages connected with the use of those lights, even on a small scale; as Mr. Murdoch had before given us a statement on a much more extensive plan, which may be found in our last volume.

Result of some Experiments on the Distillation of various vegetable and animal Substances in the dry Way. By DAVID MUSHET, *Esq.*—*Phil. Mag. Nos.* 129, 130.

THESE experiments appear to have been instituted for the purpose of ascertaining the quantity of volatile and carbonaceous matter contained in various vegetable and animal substances; the distillation of them was continued till every thing that was volatile was carried over. As the phenomena attending the twelve first are very nearly the same, the residual charcoal differing but little in its appearance, always caking, of a greater bulk than the substance from which it was derived, and varying principally in colour, we shall only note the proportions of the volatile and carbonaceous matter of which they were composed. In each experiment, 100 grains of the subject were to be employed. Raw sugar contained 85.9 volatile matter and 14.1 oxide of carbon.—Loaf sugar contained 85.26 vol. matter and 14.74 ox. of carb.—White silk stuff contained 69 vol. matter and 31 ox. of carb.—Pure white wool contained 76.11 vol. matter and 23.89 ox. of carb.—Very clean rice contained 82.5 volatile matter and 17.5 ox. of carb.—Well-dried barley contained 81 vol. matter and 19 ox. of carb.—Core of an egg dried to the consistency of a horse's hoof contained 93.7 vol. matter and 6.3 ox. of carb.—Goose feathers contained 90 vol. matter and 10 ox. of carb.—Cork contained 84 vol. matter and 16 ox. of carb.—Horse-hoof contained 86 vol. matter and 13 ox. of carb.—White horse-hair contained 81 vol. matter and 19 ox. of carb.—Black horse-hair contained 81 vol. matter and 19 ox. of carb.—The mane of the same horse afforded 74.86 vol. matter and 25.14 ox. of carbon.

The substances operated upon in the following experiments afforded a coal that retained the natural form of the body.

Swedish turnips and their skin afforded 96 of vol. matter and 4 ox. of carb.—Potatoe afforded 94.83 vol. matter and 5.17 ox. of carb.—Garden-beans afforded 87.8 vol. matter and 12.2 ox.

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of carb.—Common field-peas afforded 80 vol. matter and 20 ox. of carb.—Oats deprived of their husks afforded 79.16 vol. matter and 20.84 ox. of carb—Flax afforded 79.16 vol. matter and 20.84 ox. of carb.—Cotton cloth well bleached afforded 85.15 vol. matter and 14.84 ox. of carb.—English apple afforded 96.77 vol. matter and 3.23 ox. of carb.—Cinnamon afforded 76 vol. matter and 24 ox. of carb.—Sweet almonds afforded 86.25 vol. matter and 13.75 ox. of carb.—Bitter almonds afforded 86.66 vol. matter and 19.34 ox. of carb.—Black pepper afforded 77.91 vol. matter and 22.09 ox. of carb.—Fine black tea afforded 69.16 vol. matter and 30.84 ox. of carb.—Gunpowder tea afforded 67.5 vol. matter and 32.5 ox. of carb.—Coffee-beans afforded 81.23 vol. matter and 18.75 ox. of carb. The residual coal was salt to the taste.—Dutch cheese afforded 82.22 vol. matter and 17.78 ox. of carb. The coal was salt to the taste, and emitted during its distillation strong fumes of muriatic acid.—Scotch cheese (Dunlop) afforded 91.66 vol. matter and 8.34 ox. of carb. The residual coal was not as salt as the last.—Cheshire cheese afforded 95 vol. matter and 15 ox. of carb. The coal had a trace of salt.

The following experiments are selected from a variety made on animal substances.

Beef afforded 97 vol. matter and 3 ox. of carb.—Fat of the same beef afforded 99.29 vol. matter and 0.79 ox. of carb.—Mutton afforded 95.74 vol. matter and 4.28 ox. of carb.—Fat afforded 99 vol. matter and 1 ox. of carb.—Veal afforded 96.47 vol. matter and 3.53 ox. of carb.—Lean pork afforded 94.03 vol. matter and 5.97 ox. of carb.—A toad afforded 94.40 vol. matter, 1.26 lime, and 4.34 ox. of carb.—White snails afforded 93.82 vol. matter and 6.18 ox. of carb.—Black snails afforded 92.15 vol. matter and 7.85 ox. of carb.—Cleansed earth-worms afforded 87 vol. matter and 13 ox. of carbon mixed with earth.

Mr. Mushet observes, it is very probable from these experiments that every animal substance contains a portion of carbon, and that many lose their carbon whilst burning, by a considerable part of it being held in solution, and carried off with the hydrogene. He ascribes the different appearances of the oxide of carbon, to the different existing states of the oxides, or to the decomposition of the hydrocarbonate which most of them contain.

Observations.—These laborious experiments prove nothing but what was well known before. We do not want to be informed that vegetable and animal substances contain volatile and carbonaceous matter; we want accurate analyses, which can never be effected, nor the science ever promoted, by experiments, which, while they aim at every thing, in reality effect nothing.

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Since the accurate experiments of Messrs. Allen and Pepys, charcoal can no longer be considered as an oxide of carbon; the term is therefore improper.

Experiments relative to Coals and Coke obtained from Wood and Pit-coal. By Mr. DAVID MUSHET.—*Phil. Mag. Nos.* 127, 128.

CHARCOAL is an oxide of pure carbon, or diamond with a certain portion of oxygen. It is furnished in various proportions by vegetable and animal substances; in pit-coal, plumbago, and some other minerals, it is contained in still greater proportion than in organic bodies.

Some oxides probably contain 30 to 50 per cent. [of oxygen?].

The quantity of oxygen has not indeed been directly investigated, but a pretty correct estimate may be formed by a comparison of their carbonating effects.

Some have thought that the oxide of diamond is formed by oxygen liberated from the atmosphere, or from surrounding bodies: this probably takes place in some degree, so that carbonaceous matter prepared in contact with atmospheric air is not so highly deoxygenated as that prepared in close vessels. It is certain, however, that the oxide prepared in close vessels is not materially altered as to colour and general appearance, nor does it exist in a state of *diminution*, qu. deterioration? But its carbonating effects are increased, and it approaches nearer to the state of diamond: this seems to arise in some measure from the oxide, at a high temperature, deoxidizing itself, and becoming less combustible.

In a heat of 160° or 170° Wedgewood, the oxide, especially if in powder, becomes denser, and its black colour assumes an uncommon depth of lustre; but as its tendency to deoxidate will diminish in the ratio of its continuance, unless some new action be excited by increase of temperature, alteration of affinity, &c. there is little hope of proceeding any further than the formation of a highly incombustible coal.

In their original state, wood and pit-coal appear to be supercharged with oxygen, which is partly carried off in burning. In the open air more of the original oxygen remains fixed, but in close vessels the portion of carbon that would have been carried off by the external air, is combined with the original oxygen, so that the oxide is left in a state of comparative purity.

All oxides of carbon, even the diamond itself, contain foreign matter in the state of earths or salts. Thus oxide [i. e. charcoal] of walnut-wood contains 3.952 per cent. of ashes; of elm, 3.3; of holly, 5.848; of Scotch fir, 2.9; of beech, 4.8; of American maple, 3.86; of mahogany, 3.846; of sallow, 6.135; of American black beech, 4.821; of Norway fir, 1.821; of ches-

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nut, 1.8; of laburnum, 4.8; of oak, 1.665; of ash, 4.273; of birch, 10.309; of sycamore, 5.406; of lime, 3.679.

The coals found in the extensive coal country about Glasgow, are divided into five workable measures, known by various local names; but the same measure always contains two, and not unfrequently four different qualities of coals: it is therefore better to class coals according to these various qualities.

Soft coals, not caking and leaving a white ash, are called commissary coal, upper coal, double coal, &c. The oxide of these coals or their cokes, contained from 2.8 to 4.05 per cent. of ashes.

Soft coals, not caking, and leaving a red ash, are frequently taken from the same measure, and even in contact with the former. The oxide of these coals contained from 25.5 to 37.15 per cent. of ashes.

Soft caking coals generally leave a red, or brownish red ash. Those found in Yorkshire yield the whitish ash, and their oxide contained from 2.55 to 8.15 per cent. of those ashes.

Soft caking coals leaving an ash of any intermediate shade between a light ochrey brown and a deep red brown, contain still more ash. The Newcastle and Sunderland coals are chiefly of this description. The oxides contain from 2.85 to 34.66 per cent. of ash; the proportion of the latter being greater as the colour of the ash was deeper. One specimen of oxide yielded 48.50 per cent. of sulphuret of coal and bitumen.

Mixed coals are in general less dissimilar from each other, and their ash is in common of a less specific gravity than that of other coals, and varies from a blueish white to a primrose colour. Their oxides yielded from 1.25 to 7.5 per cent. of ashes.

Hard coals may be divided into three varieties, viz. candle or cannel coal, hard coal, and stony hard.

The oxides of candle coal yielded from 7.54 to 41.75 per cent. of pure white ashes.

The oxides of hard coal are considerably dense, and yield from 6.78 to 12.12 per cent. of ashes, which were generally of a pure white colour.

Stony hard coal yields after combustion a white stone covered with very fine white ashes. The coke contained from 15.75 to 51.90 per cent. of ashes.

Observations.—The experiments lately made by Mr. Davy have shewn that charcoal and cokes are not oxides of carbon, as here denominated by Mr. Mushet, but that they are hydrogurets of that substance.

Mr. Mushet does not notice the experiments of Messrs. Allen and Pepys, which shew that charcoal and cokes are not *oxides* of carbon as stated by Mr. Mushet, but that they require nearly the same quantity of oxygen for their combustion as the diamond.

Mr. Davy's recent experiments seem to warrant the conclusion

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that the diamond is not a simple substance; but, that it contains "foreign matter in the state of earths or salts," as Mr. Mushet affirms, is a mere assumption, unsupported by a single fact.

It is well known that the ashes of vegetables afford earthy and saline matter, but this is no proof that their carbonaceous principle contains these substances.

Experiments on various Earths, undertaken with the View of ascertaining whether they are metallic Oxides. By DAVID MUSHET, *Esq.—Phil. Mag. No.* 130.

MR. Mushet about eight years ago was engaged in a series of experiments to metallize the earths; and although he repeated them several times, he obtained no perfect results.

His first experiments were made with pure earths, clay, silex, lime, barytes, and strontian; and considering them as metallic oxides, he attempted to reduce them by long cementation, and by exposing them to a strong heat imbedded in charcoal. He found that clay and silex, when freed from charcoal, were infusible in the highest heat that could be urged in a wind furnace. Barytes, lime, and strontian entered into fusion with various proportions of charcoal, but they did not, like iron ore, when treated in the same way, lose any of their weight. The glasses they afforded were either white, opake, brown, or black. Barytes was the only one which appeared to have undergone any change; on the surface of its glass there constantly appeared a pellicle strongly alkaline. From Mr. Davy's late discoveries he was afterwards led to conclude that this pellicle was barytium, which, by attracting oxygen from the atmosphere, had returned to an alkaline earth.

Mr. Mushet some time afterwards found that those glasses which he had laid aside for further examination, were converted into a fine powder, and that the same thing had happened to a glass of mangauese; hence he infers, that in the original experiment a deoxidation had taken place, and that the present effect had been produced by a reassumption of oxygen.

This phenomenon induced him to make a set of comparative experiments by fusing pure malleable iron with the different earths, in order to ascertain the quantity of oxygen that the iron would absorb, and hence obtain a knowledge of the comparative quantity of oxygen contained in each of the earths.

1. Two hundred grains of calcareous earth (very pure Paris white) deprived of its carbonic acid, were brought into perfect fusion with 50 grains of iron filings: a black glass was produced of a deep jetty lustre. Two finely polished spherules of metal were obtained, weighing 12 grains—loss by oxidation 38 grains, or 76 per cent. Fifty grains of the same iron were fused *per se* in ten minutes, the button formed weighed 47½ grains—loss only

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7½ per cent. Oxidation in consequence of the calcareous earth, nearly 70 per cent. more.

2. In the same process barytes being substituted for lime, Mr. Mushet found that 100 grains of barytes oxidated 41½ grains of iron, whereas calcareous earth only oxidated 19 grains.

In several similar experiments on the earths, and on other bodies, Mr. M. conceived that barytes contained either a greater quantity of oxygen, or gave it out to iron with the greatest facility. Hence he fixed upon it as the best subject for the next experiment.

One hundred grains of pure barytes were fused with 200 of iron-stone and 20 of charcoal (the iron-stone was used for the purpose of depriving the pellicle, formed on the surface of the glass, of its oxygen by the means of a second affinity). A button of iron was found weighing 33 grains; this was surrounded by a black glass which contained no barytium; its surface was covered with a metallic crust, crystallized in small concentric radii, inclining to a brownish silvery colour; it brightened a little under the file. It had a metallic appearance, and was superior to any of his former results in respect of continuity of grain and lustre.

Two hundred and eighty grains of an ore of iron composed of 90 parts of iron with which a little oxygen was combined, and 10 parts of lime, were fused together; the result was a double stratum of reguline matter, the upper of which was more metallic than the lower. The button of iron was covered on its upper surface with a silvery white circle, delicately crystallized in the starry form peculiar to the barytium; and because Mr. M. had never seen this before, he was led to believe that it was an alloy of the iron and of the metal of barytes. In this experiment 97$\frac{2}{10}$ of iron were oxidated by 280 grains of barytes.

Mr. Mushet has not been able to ascertain any thing decisive respecting silex and clay considered as metallic oxides. He does not think from his experiments that they are in the most distant manner allied to the other three earths, though they may be more akin to each other. He concludes his communication in these extraordinary words: "It is possible that silex may prove to be clay completely deprived (or nearly so) of all its moisture. Or, in other words, that clay, by fire or other natural processes, becomes oxygenated to such an extent as to convert it into what we call silex. May clay not prove to be water and oxygen, and silex, this oxygen without water?"

Observations.—We have only selected a few of the numerous experiments of Mr. Mushet, as specimens of the rest. For it would, we fear, only tire the patience of our reader to peruse experiments which Mr. M. himself confesses led to nothing like a perfect result. Indeed the motive which induced Mr. Mushet to publish these experiments was laudable: he wished to deter

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others from pursuing a course which, though attended with much labour, was productive of little real advantage.

Among the pure earths Mr. M. notices *clay*; we suppose he means what in the modern nomenclature is termed *alumine* or the pure earth of clay, for clay is certainly a heterogeneous body. We cannot conceive what could have induced Mr. M.'s very singular and extraordinary hypotheses relative to the nature of clay (*qu.* alumine?) and silex. We see no reason to adopt an opinion which is opposed to all experiments, to all analogies, and which can only be regarded as vague and idle speculation.

On the Difference between the Products obtained by Distillation of recent and of dried Vegetables. By Mr. GARDEN.—*Phil. Mag. No.* 130.

WHEN the recent leaves of vegetables are exposed to a low degree of heat, they undergo sensible changes; their bulk is much diminished, their colour less vivid; if they have any fragrance, it is either impaired or destroyed; and they lose considerable weight.

These changes chiefly depend on the evaporation of aqueous moisture, dissipation of the aroma, and loss of a portion of the volatile or essential oil. Some practical operators contend, that this last effect does not take place, and that dried vegetables furnish more essential oil by distillation, than when in a recent state.

Mr. Garden states, that it is not his intention to combat this opinion, or to give any detail relative to the chemical nature of vegetable bodies, but simply to notice a fact which he has lately observed.

A quantity of the dried leaves of peppermint (*mentha piperita*), which had been kept in well-closed casks for nearly two years, and apparently in good preservation, were exposed to distillation with a view to obtain the essential oil. A common still, with a connecting refrigeratory, was used, and the products received in an Italian recipient, usually employed to obtain the lighter essential oils. When a quantity of the fluid had distilled, and what remained tasted little of the plant, the process was stopped. The essential oil produced was very little, though nearly forty pounds of leaves were used.

The failure of this experiment was supposed to have been owing to some mismanagement. It was again repeated on a similar quantity of the leaves, and every precaution used to ensure success. The quantity, however, was nearly as in the other process; and several subsequent distillations seem to prove, that dry plants do not furnish so much essential oil as when in a recent state.

In these operations the deficiency of essential oil appeared to be balanced by a considerable quantity of water, highly impregnated

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with the taste and flavour of the plant. This circumstance seems to warrant the presumption, that the herb, though confined in the casks so long, had not suffered so much deterioration as the deficiency of oil indicates; for though the essential oil did not appear in an uncombined state, it probably underwent some modifications which rendered it more soluble, and hence the increased quantity of highly impregnated water.

The addition of minute quantities of alkaline substances has been said to assist the separation of volatile oils from their bases. This, however, was tried without effect.

Observations.—The quantity of essential oil obtained by the distillation of vegetables seems to depend on the temperature to which they are exposed. In general, it should scarcely exceed the boiling point of water.

The results in these experiments are such as might naturally be expected: for vegetables in the act of drying undergo partial decomposition, and the loss of the principles essential to the formation of oil must necessarily be connected with a diminution of the quantity obtained.

The opinion that dried vegetables furnish more oils than when in their recent state, appears to be inconsistent with a knowledge of chemical physiology.

On the supposed universal Distribution of fossil Coal, in Reply to Mr. B. Cook; and on the Nature and Situation of the extraneous Fossil (Belemnite). By Mr. JOHN FAREY.—*Philosophical Journal, No. 96.*

MR. B. Cook asserted in a former communication (see our last volume, p. 438), that fossil coal might be found almost every where in this country: Mr. Farey considers this statement as erroneous, and observes that the opinion has caused the useless expenditure of large sums of money; he instances a recent case of this kind in Sussex.

Coals are only found in particular districts. There is one passing from Somersetshire, through Gloucestershire, Warwickshire, Leicestershire, Nottinghamshire, Derbyshire, Yorkshire, and Durham. Some extensive strata also occur to the westward and northward of this line; but to the eastward or southward of this line no coal has ever been discovered at practicable mining depths, though speculators, by confounding bituminated wood with fossil coal, have in this respect often been deceived.

The substance called *thunder-pick*, analysed by Mr. Acton, is not a crystal, as Dr. Woodward supposed, but the exuviæ of the *belemnite*, an animal now unknown. It is often found among alluvial matter in ploughed lands, and exists in quantity in a clay stratum under the Woburn sand, and also in a stratum in the Bath freestone hills.

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On the Decomposition and Recomposition of Boracic Acid.—By Messrs. GAY-LUSSAC, and THENARD.—*Journ. de Phys. Nov.* 1808.

IT was announced, on the 21st of June last, in a note read to the Institute, that, on treating fluoric and boracic acid with the metal of potash, results were obtained, which shewed that they were composed of a combustible base, with oxygen. These researches have since been continued with still greater success.

Boracic acid may be decomposed, by heating equal parts of the metal of potash, and pure melted boracic acid in a copper tube, to which a mercurial pneumatic apparatus was adapted. The decomposition was finished in a few minutes, after the tube had become red.

As soon as the tube was heated to 150° Cels. (312° Fahr) the mixture became suddenly ignited, and so much caloric was disengaged, that if a glass tube was used, it was partly melted, and frequently broke. Only a few bubbles of hydrogen were emitted, not the 50th part that the metal would have separated from water. An olive grey substance was left, the alkali in which was saturated with muriatic acid, and then separated by heating water in the tube.

The residual boracic radical, was greenish brown, fixed, insoluble in water, tasteless, and having no action upon tincture of litmus, nor syrop of violets. Projected with oxymuriate, or nitrate of potash, into an ignited crucible, it yielded a flame, and produced boracic acid. This acid was also obtained, by treating the radical with nitric acid.

Three decigrammes (4½ grains) of boracic radical, was placed upon a silver crucible, heated to a dark red, and then introduced into a jar, containing a litre (quart) of oxygen, standing in a mercurial trough, a most rapid combination took place, and much of the gas was absorbed; but the radical was not entirely acidified, because it first passed to the state of a brown oxide, and then the external part becoming acidified, melted by the heat, and defended the internal from the oxygen; so that it required several successive exposures to oxygen, and alternate washings of the oxide with warm water, to separate the acid, before the whole could be acidified.

The combustion is less rapid when common air is used instead of oxygen.

The basis of the boracic acid is intended to be called (bore) borium; and it may be classed along with phosphorus, carbone, and sulphur.

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On the Fluoric Acid.—By Messrs. GAY-LUSSAC, and THENARD. *—Journ. de Phys. January*, 1809.

THE dry fluoric acid gas, disengaged from fluate of lime, and melted boracic acid, produces thick vapours with every gas, except muriatic acid gas, unless they have been left for some time on lime, or muriate of lime to dry them. If the gases are moist, the mixed gas loses a few hundredth parts of its bulk, so that fluoric acid gas is an excellent test for the hygrometrical trial of a gas.

Neither muriatic acid gas, nor fluoric acid gas, nor probably ammoniacal gas contain any water, and therefore they do not deposit any moisture, like carbonic acid gas, sulphurous acid gas, when exposed to cold.

It is probable, that water has a stronger affinity for fluoric acid, than for the muriatic; and would absorb more than 2000 times its bulk of it. The water thus saturated, forms a limpid, smoking, and excessively caustic acid, from which only one fifth of the gas can be separated by heat. It has the appearance of sulphuric acid, does not boil but at a temperature above boiling water, and distils over in striæ, although it then contains 1600 times its bulk of gas; so that it is even probable, that sulphuric, and nitric acid, owe their liquid form, to the water they contain, and would form gases if separated from it.

The gas, disengaged from the above dry materials, did not absorb any water, when a drop of water was exposed to a large quantity of gas over mercury, so that it does not hold any water, either combined, or hygrometically. The experiments of Berthollet, the Son have shewn, that ammoniacal gas, holds no combined water, and probably no hygrometric water. Muriatic acid gas does not contain hygrometric water, but, as has been shewn by Henry and Berthollet, it contains combined water. This may indeed be rendered visible, by passing muriatic acid gas through litharge, melted and reduced to coarse powder. The water forms one fourth of the weight of the gas, according to some experiments, in which the gas was combined with an excess of oxide of silver. The other gases, contain only hygrometric water.

It is a very striking phenomenon, that muriatic acid gas contains so much water, that when it is passed through ignited gun-barrels, filled with turnings of iron, it is totally absorbed by the oxide, and forms a muriate. Perhaps hydrogen, and oxygen, are two elements of the muriatic acid, and do not exist in it, in the form of water, which is only formed when the acid enters into combination. It is certain, that those muriates, which cannot be decomposed by heat, and contain little, or no water, cannot be decomposed by the acid phosphate of lime, nor by boracic acid;

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and it is even probable, that if the sulphuric acid was deprived of water, it would not decompose those muriates.

To return to the fluoric gas, it acts on vegetable substances, and carbonizes them, while water is formed. It forms an ether, with alkohol, and immediately blackens the dryest paper, vapours being produced. It has no action upon glass, which appeared to arise from its containing a large proportion of boracic acid.

When fluate of lime is decomposed by acid phosphate of lime, very little gas is obtained, and this gas held in solution, some silica from the fluate that was employed, and also some acid phosphate. When a siliceous fluate is employed, the decomposition, is more rapid than usual.

When fluate of lime was decomposed by sulphuric acid, in leaden vessels, no gas was obtained, but a smoking liquid, which became hot, and even boiled when added to water. It immediately roughened glass, became very hot, boiled, and was reduced to siliceous gas. Its action upon the skin, was excessively rapid and strong; the least drop produced a white spot, so exceeding painful, as to produce febrile symptoms, which afterwards suppurated. To prevent this ill effect, a weak solution of caustic potash, which is an excellent remedy for burns, was applied, as soon as possible.

A small quantity of the metal of potash, flung into this acid, produced a most violent explosion. By dropping, however, a little of the acid upon the metal, an increase of temperature took place, but the products could be collected, which were hydrogen, fluate of potash, and water. Of course, the active liquor, was merely fluoric acid and water.

The fluoric acid, is the only acid that forms solid, liquid, and gaceous combinations with different bodies; which shews it is the strongest, and most active of all. It cannot be obtained quite pure, therefore a choice must be made amongst the combinations of that, which is the most proper for the intended purpose.

Siliceous fluoric acid, ought not to be employed to act upon alkalies, earths, or metallic oxides, because it would produce triple salts. Thus if ammonia is poured into acid fluate of silica, a triple salt is obtained, which is almost insoluble, and yet, for the most part volatile. Muriate of barytes poured into acid fluate of silica, yields after some time, a crystalline precipitate, that is insoluble in nitric acid, and might be mistaken for sulphate of barytes, but which is a fluate of silica—and—barytes.

To decompose, however, the fluoric acid, the fluoric gas holding boracic acid, or rather siliceous fluoric gas ought to be used, because they do not, like the liquid acid, hold any combustible body in solution.

The metal of potash, put into siliceous fluoric gas, became only tarnished, until it was heated to fusion, and then it burned very violently, a large quantity of the gas was absorbed, very lit-

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tle hydrogen was disengaged, the metal disappeared, and a reddish brown substance was left, which being treated with water, afforded some hydrogen, but not one third of what the metal would have yielded. The water held in solution an alkaline fluate of potash. The residuum was still reddish brown, it burned vividly at a low red heat, emitting a little acid gas, and left a solid mass, consisting partly of siliceous fluate of potash, and partly of fluate of potash, which last was separable by washing. When this combustion was made under a jar, much oxygen was absorbed. As little, or no hydrogen was disengaged during the combustion of the metal in fluoric gas, either the fluoric acid was decomposed, or it combined with the metal, without oxygenizing it; but it is most probable, that the acid was decomposed, and that the residuum was a compound of the radical, with potash and silica. The combination of the radical with potash, appears to decompose water, as is done by phosphurets; but the combination of the radical with potash, and silica, being insoluble, had not that effect.

It is easy enough to burn the metal of potash in fluoric gas, by putting in small quantities at a time, and heating it, either by a hot coal held near the jar, or by using a heated capsule. It is necessary, however, to wipe very carefully, the surface of the metal of potash, with blotting paper, in order to separate the oil, which would otherwise confound the operator, by yielding hydrogen and carbone, but with every care some will remain, and to this it is owing, that the metals of potash and soda, do sometimes render lime water turbid.

Action of the Metal of Potash, on Metallic Oxides and Salts, and on Alkaline and Earthy Salts.—By Messrs. THENARD, and GAY-LUSSAC.—*Journ. de Phys. January*, 1809.

It being impossible to obtain muriatic acid, exempt from every other substance, the action of the metal of potash upon the muriates was examined.

Metal of potash had no action upon muriate of barytes, that had been previously melted by a red heat, but, on adding water, it burned very vividly. Other alkaline muriates did not give more satisfactory results. With muriate of silver, and calomel, it burned very vividly, the salts were reduced, the tubes were broke, muriate of potash was formed, but no decomposition of the muriatic acid could be discovered; with calomel, a slight explosion of the mercurial vapour took place.

The same process was tried with other salts, and oxides, the heat being rather more than was necessary to melt the metal; and in some cases, as in decomposing phosphate of lime, sulphate

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of barytes, oxide of zinc, &c. it was raised to 300° Cels. (572° Fahr.) and whenever the combustion was very brisk, the tubes were broken.

Decomposed without flame. Sulphate of barytes, yielding sulphuret of barytes; nitrate of potash, probably because it contained water; phosphate of lime, phosphuret of lime being produced; carbonate of lime, yielding charcoal; chromate of quicksilver, but became slightly red; black oxide of iron, which was reduced; oxide ad mininum of manganese; white oxide of zinc; green oxide of chrome, which yielded a blackish substance, of a pyrophoric nature, leaving on combustion, a yellow residuum of chromate of potash.

A slight flame. Sulphite of lime, yielding a very yellow sulphuret; red oxide of iron; volatile oxide of antimony; white oxide of arsenic; black oxide of cobalt.

A brisk flame. Sulphate of barytes, yielding sulphuret of barytes; chromate of lead; arseniate of cobalt; green and yellow tungstic acid; red and yellow oxide of lead; yellow and brown oxide of copper; common oxide of tin; yellow oxide of bismuth; grey oxide of nickel.

A very brisk flame, sulphate of lead; sulphate of slightly oxidized quicksilver; nitrate of barytes; oxymuriates; red oxide of quicksilver, with a slight explosion of the mercurial vapour; oxide of silver; brown oxide of lead; oxide of antimony ad maximum; oxide of tin ad maximum; oxide of manganese ad maximum.

The metal of potash was evidently altered by the earths, including silica, zircone, ittria, and barytes; but the cause of this alteration is not well known. It is however probable that the phenomena observed in burning potash in siliceous fluoric gas are not owing to the silica.

It appears certain that all substances known to contain oxygen, are decomposed by the metal of potash, light and heat being disengaged more or less as the oxygen is more or less condensed, so that this operation furnishes a means of appreciating the condensation of oxygen in any body.

Mr. Descotils has found that boracic acid may be decomposed by heating a mixture of it with charcoal and iron or platina in a forge, as the buttons which are obtained yielded a sensible quantity of boracic when treated with nitromuriatic acid; so that they were certainly combinations of (bore) borium with iron or platina.

On Muriatic Ether.—By Mr. THENARD.—*Journ. de Phys. August* 1808.

It was mentioned in the former paper of the author on this subject (see Retrospect, vol. iii. p. 436.) that he meant to prose-

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cute his researches, and to determine the quantity of muriatic acid, or of its elements that entered into the composition of muriatic ether.

Fifty three thousand four hundred and seventy six centigrammes (18¾ oz.) of muriatic acid, and an equal bulk of alkohol were poured into a retort, and distilled. The gas was passed through 2 litres (4¼ pints) of water, and received in a trough and jars containing 228 centilitres (4½ pints) of water. By this means 3814 centilitres (80½ pints) of ethereal gas, was produced, including that portion which was absorbed by the water, or left in the apparatus.

In this operation 17621 centigrammes of the acid disappeared; a quantity sufficient to have saturated 10078 centigr. of dry potash and to have formed 131018 centigr. of melted muriate of potash, so that the ethereal gas contained 3024 centigr. of dry acid. This gas at a pressure of .75 metre and at the temperature of 18° Cels. weighed 102722 centigr. so that its specific gravity to air was as 2.219 to 1, and it contained more muriatic acid than an equal weight of muriate of potash.

It was very difficult to examine how much hydrogen, carbone, or oxygen were combined with the acid so as to form this gas, because the detonation of it with oxygen repeatedly reduced the upper part of the eudiometers to a mere powder, so that it was necessary to enclose one in a copper case.

On a mean of 4 experiments it appears that 66 measures of ethereal gas detonated with 224.35 of oxygen produced 135.75 of carbonic acid, and left a residuum of 13.5 of oxygen. The barometer being at 0.767 metre, and the thermometer at 18° 88 Cels. From which it may be concluded that 14172 centigr. of muriatic ether contains 4172 centigr. of muriatic acid, 5189 of carbone, 3303 of oxygen and 1508 of hydrogen. It must however be observed that no notice is here taken of the aqueous vapour.

No difference was observed in analysing the ethereal gas that had not been absorbed by water, or that which had been previously absorbed and afterwards expelled by diminishing the pressure.

Whether it is alkohol, or only some of the elements of alkohol that combine with the acid to form the ether is difficult to determine, because the separation of the acid by means of potash does not succeed.

When the mixture of acid and alkohol is distilled, no other gas but the ethereal is obtained; and, even when 2 chilogrammes (4 lb.) of the mixture was distilled to dryness, only very slight traces of a blackish residuum are discoverable, so that all the carbone of the alkohol enters into the ether. Is is doubtful whether any of the hydrogen and oxygen are expended in the formation of water, notwithstanding Saussure's statement of the elements of alkohol, because the purest alkohol still contains water. The azote stated by Saussure to exist in alkohol may perhaps be

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changed during the detonation of the ethereal gas into nitric acid, a point which requires examination.

There is still greater difficulty in determining how the elements of muriatic ether are united, whether the elements of alkohol are previously disunited or exist in the state of alkohol, and whether the muriatic acid is decomposed or not.

It was observed in the former paper that muriatic ether does not redden litmus, nor precipitate nitrate of silver, nor is it decomposed by the alkalies, and yet, on being burned it yields so much muriatic acid as to precipitate concentrated nitrate of silver in a solid mass. Fresh experiments have been made on this subject as follows, the mixtures being left from the 21 Feb. to the 19 May.

Solid caustic potash has no action on gaseous or liquid ether; when added to water containing ether; the temperature was increased and the ether disengaged.

Solution of caustic potash has no action upon gaseous ether, but the potash acquired some trace of muriatic acid. It was but slowly acted upon by liquid ether, which was not sensibly diminished and the potash remained very caustic, but it acquired in a day's time some traces of muriatic acid.

Solution of nitrate of silver exhibited no trace of any precipitation by gaseous ether until an hour had elapsed; even at the end of the three months, it was very slightly affected. Nearly the same effects took place with liquid ether, and water holding ether.

Solution of nitrate of slightly oxidized quicksilver, exhibited nearly the same effects as the preceding nitrate.

Concentrated sulphuric acid had no action on gaseous, or liquid ether; water holding ether acquired an increase of temperature, and the ether was disengaged, but no muriatic acid was separated.

Concentrated nitric acid had no action on gaseous or liquid ether: neither are they affected by the nitrous acid.

The ethereal gas, passed through boiling sulphuric, or nitric acid, is decomposed, and much muriatic acid is obtained.

The liquid ether was decomposed by oxymuriatic acid gas, the temperature was greatly increased, and a great quantity of muriatic acid was disengaged.

Muriatic ether was also treated with potash, ammonia, &c. in different manners.

Ethereal gas was passed for 15 hours through 160 grammes (56 oz.) of a solution of caustic potash, heated by degrees to 80° Cels. (176° Fahr.) the gas was not altered, nor diminished in bulk; the potash contained about 4 decigr. (6 grains) of muriatic acid, but no alkohol. A few drops of water was also collected in the next bottle.

Sixty grammes (2 oz.) of alkohol, sp. grav. 800, was saturated with caustic potash, and 15 gr. ($\frac{1}{2}$ oz.) of liquid ether, was then added. At the end of 2 hours, some traces of acid were discove-

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rable; the next day, muriate of potash began to be deposited, and at the end of 8 days, this deposit became considerable; nevertheless, the ether was still separated in great quantity, on adding water.

Two litres (quarts) of ethereal gas were placed in contact with 60 grammes (2 oz.) of liquid ammonia. The gas was dilated, and in an hour, some traces of muriatic acid were perceptible. In 4 days, a slight absorption had taken place, and about 2 decigr. (3 grains) of muriatic acid were separated by nitrate of silver.

Twelve grammes (184 grains) of liquid ether, were added to 30 (1 oz.) of liquid ammonia, and frequently shook together; at the end of three days, they still formed separate strata; 4 decigr. (6 grains) of muriatic acid was separated, but no alkohol.

Equal bulks of ethereal gas and ammoniacal gas, did not form vapours on mixture, nor did the bulk of the mixture diminish in 4 days; very little muriatic acid was separated.

In the former memoir it was observed the ethereal gas passed through a glass tube heated cherry red, deposited little or no charcoal, while nearly the whole of the muriatic acid was separated, and that a considerable quantity of very ponderous difficultly combustible, elastic fluid was disengaged, which certainly consisted principally of charcoal. If the tube is ignited by a greater heat, so much charcoal is deposited that it is almost immediately stopped up, and an explosion takes place. With a copper tube 0·025 metre (1 inch) in diameter the operation went on for some time, but at length the retort attached to the tube burst, which might probably have been avoided by adding a bent tube to the opening of the retort. It appeared however that muriatic acid was developed in great quantity, and the inflammable gas was very light.

Analysis of Aplome.—By Mr. LAUGIER.—*Journ. de Phys. Nov.* 1806.

Mr. Weiss having sold his collection of minerals to the French government, it appeared from his specimens that aplome was found on the banks in the Lena in Siberia.

Aplome loses 2 per cent by calcination. Melted with 4 times its weight of potash; it formed a bottle-green glass. It contains 40 per cent of silica, 20 of alumine, 14.5 of lime, 14.5 of oxide of iron, 2 of oxide of manganese, and 2 of a combination of silica and iron. 2 were lost, as is said above, by calcination, and 5 by the subsequent operations; so that it cannot be referred to any known species of minerals, and is probably a new one.

Observations.—Lucas observes that aplome is found in several collections under other names; and in particular it is confounded among garnets as a green variety of them.

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On the Bile. By Mr. THENARD.—*Journ. de Phys. Sept.* 1807.

GREAT difficulties and many disagreeable circumstances occur in the analysis of animal substances: but the importance of the bile to the animal œconomy has caused it to have been examined with great care by most physicians, and yet there remains many important observations to be made on it.

Oxgall is usually greenish yellow, seldom dark green; it changes tincture of litmus and syrop of violets to a reddish yellow, but this is merely owing to its own colour. It is very bitter, and at the same time slightly sweet. In smell, it resembles some fat substances when hot. Its spec. grav. is about 1.026 at 60° Cels. or 42° Fahr. when it contains about 8-9ths of water.

It sometimes has the consistence of a mucilage, and is sometimes thicker. It is sometimes transparent, and at other times rendered turbid by a yellow substance separable by water. It is erroneously supposed to be of a saponaceous and albuminous nature.

When distilled, it becomes slightly turbid, throws up a quantity of scum, and there passes over a colourless liquid, which retains the smell of the bile even when redistilled. This liquid has a faint taste, and precipitates acetate of lead of a white colour.

The dry residuum is about 1-8th or 1-9th of the gall. It is yellowish green, very bitter, slightly deliquescent, and entirely soluble either in alkohol or water. It melts by a slight heat, and is decomposed by a greater, yielding more oil and less carbonate of ammonia than most animal substances, and a very bulky charcoal, the calcination of which requires great care.

As only about 1-200th of soda exists in the bile, it was not likely that this small proportion of alkali would dissolve the resin, and therefore some other substance probably acts as an alkali in the bile.

A very small quantity of acid added to oxgall does not render it turbid, but it then reddens litmus paper or its tincture; a larger quantity of acid throws down a precipitate, which is more abundant if sulphuric acid, rather than nitric or any other acid, is used. The precipitate is composed of yellow animal matter, and a little resin; but the supernatant liquor still retains the greater part of these substances. Nevertheless these very substances, when dissolved in soda, are totally precipitated even by acetous acid.

Some other substance, therefore, is found in bile, which was very difficultly separated; at last it appeared that common acetate (sugar) of lead threw down a very abundant yellowish white preci-

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pitate from oxgall, which was separated by filtration, and sulphuretted hydrogen passed through the clear fluid to get rid of the excess of lead. And now the liquor being evaporated, left a light-coloured sticky substance, about 4-5ths of what would have left by the oxgall itself. It was slightly saccharine, sharp, and very bitter, neither decomposed by acids nor by most metallic salts; it dissolved a large proportion of resin of bile, and then had the same qualities as bile itself.

This viscous substance being bitter, appeared to retain much of the resin, which latter has a strong affinity for oxide of lead. On using, however, lamellar acetate of lead, not only the resin was precipitated, but also a great proportion of this new substance. But on using a solution of 8 parts of common acetate of lead and 1 of oxide of lead, then treating the residual liquor with sulphuretted hydrogen, and afterwards evaporating it to dryness, a sharp, but sweetish, substance was obtained, not absolutely pure, yet far less bitter than before. This substance retained some acetate of potash, which was separated by lamellar acetate of lead, an insoluble combination of oxide of lead and this new substance was obtained, which was soluble in vinegar, and from this solution the lead was precipitated by sulphuretted hydrogen, and the acid driven off by heat.

To this substance the name of picromel (bittersweet) was given on account of its taste. It favoured the solution of the resin. By dissolving the mixture of these substances in alkohol, and after evaporating the liquid, treating it afresh with water, it appeared that 3 parts of picromel dissolved 1 of resin of bile, and that by employing 2 of picromel and 1 of resin, a solution was made in a very little water, and it was rendered turbid if more was added. Hence the picromel is not sufficient to dissolve all the resin in the bile, which is frequently not rendered turbid by acids; and it seemed as if some soda was combined with the resin and picromel. The extract of oxgall acidified by sulphuric or muriatic acid being calcined contained carbonate of soda, but less in quantity than extract of pure bile.

By mixing, therefore, with picromel, resin, and yellow matter the necessary quantity of soda, a liquid was obtained similar to the gall itself.

The proportion of the principles in bile was determined by separating the yellow matter and some of the resin by nitric acid, and parting them by dissolving the latter in alkohol. The remaining liquid was precipitated by common acetate of lead, to which 1-8th of litharge was added. The resin was separated from this precipitate in the form of soft green lumps by weak nitric acid. Sulphuretted hydrogen was passed through the remaining liquor to separate the lead, and it was then evaporated to dryness, the residuum was weighed, and the weight of the

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acetate of soda, formed by adding the acetate of lead, being substracted from it, gave the weight of the picromel. The weight of the acetate of soda was obtained by calcining a known quantity of extract of bile, elixiviating the residuum, and pouring acetate of lead with excess of oxide into the liquor, then filtering it, and passing sulphuretted hydrogen through the clear liquor, which was then evaporated to dryness.

The quantity of the saline substances was determined by calcining the extract of bile, and elixiviating the ashes, to separate the phosphate of lime and oxide of iron. The filtered liquor was saturated with nitric acid, whose strength was previously determined in order to ascertain the quantity of soda it would saturate. The other salts were determined in the usual manner.

Thus 800 parts of oxgall were found to contain about 700 of water, 24 of resinous matter, 60.3 of picromel, about 4 of yellow matter, 4 of soda, 2 of phosphate of soda, 3.2 of muriate of soda, 0.8 of sulphate of soda, 1.2 of phosphate of lime, and some traces of oxide of iron.

The picromel is soluble in water, or alkohol; it does not crystallize, nor ferment; it precipitates nitrate of quicksilver, the solutions of iron, and acetate of lead with excess of oxide. It forms a triple combination with soda and the resin, that is not decomposed by acids, the neutral or earthy salts, nor by many other bodies.

The resin or oily matter is solid, very bitter and green, it gives its own colour, taste and smell to the bile; when melted it turns yellow. It is soluble in alkohol, from whence it is thrown down by water. It is very soluble in the alkalies, and separable from them by acids. When boiled in water, it is not dissolved, but sulphuric acid renders the water slightly turbid, the other acids do not produce this effect.

This resin has a great resemblance to an oily and excessively bitter substance which was obtained by treating silk with 4 times as much nitric acid; it precipitated in flakes on evaporating the liquor. This was not the bitter matter of Welter, for it melted upon ignited charcoal, and was volatilized with flame.

Muscular flesh was distilled with 4 times as much nitric acid until no more azote was disengaged, and the residual liquor saturated with potash and evaporated to separate the new formed nitrate of potash. The mother water was precipitated by acetate of lead with excess of oxide, and the copious yellowish white precipitate treated with warm dilute sulphuric acid: by which means a very deep brown liquor was obtained. This liquor yielded in evaporation an insipid substance, not crystallisable, very soluble in water, not coagulable by acids, not reddening tincture of litmus, affording a copious precipitate by acetate of lead with excess of oxide, and which on being dried on a sand bath became

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suddenly changed without taking fire and formed a very light charcoal. Sometimes this carbonization was less sudden, the substance being probably less oxygenized. This substance is undoubtedly a new principle, and remarkable for the quantity of oxygen that it contains. Many similar discoveries might be made by attentively examining the products of the action of nitric and other acids upon animal substances.

The yellow matter is not albumen, nor fibrine, as supposed by Van Bochante; it is putrescent, and the origin of the calculi found in the gall bladder of oxen, while those in the human subject are formed from the resin. It is rendered soluble by the soda, or, perhaps, by the triple combination of soda, picromel, and resin, from which it is precipitated by acids.

The soda is probably in the state of subcarbonate, for on adding an acid to the bile, a gas is evidently disengaged.

As for the salts of soda, phosphate of lime, and the oxide of iron, they may be regarded as merely accidental.

The properties of bile are easily deducible from the knowledge of its ingredients, and it is evidently the picromel that communicates a saponaceous character to the bile.

Many interesting researches still remain to be made on this subject; it is already known that there is a great difference between the human bile and oxgall. The human bile itself is not always alike; it becomes insipid and albuminous when the liver is changed and become fat.

On Crystallography.—In a Letter from Mr. D'—— to Mr. BERTHOLLET.—*Journ. de Phys. Juillet*, 1807.

Although there remains much to be done in regard to the analysis of minerals, yet the composition of one half of mineral substances is well ascertained. If several specimens of some minerals known by the same name, present differences in their composition, it can only be said, that the elements of these bodies do not occur in any fixed proportion. Vauquelin has shown that terre yerte contains iron chemically combined; and the same may be said of the iron in hornblend, augite, epidote, &c., although its proportion is very different in different specimens of these stones.

The present analytical expression of a mineral, cannot indeed be regarded as the true expression of its composition, but as merely relative, yet this relative composition depends more upon the essential properties of the mineral than the slight differences in transparency, hardness, or the neatness of the natural joints, and it is therefore the principal thing to be regarded in the classification of minerals.

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Even the form of the molecules, or rather the cleavage is of far inferior value. The most experienced crystallographers would be found to assign very different primitive forms to the same substance.

Werner first noticed the cleavage of minerals as a natural character, but Hauy greatly extended his ideas on this subject. Since the publication of Hauy's *Traité de Minéralogie*, the mechanical division of minerals has been pushed to an extreme, so as to discover a multitude of new joints. Instead of the 3 joints assigned by Hauy to calcareous spar, 16 have been found; 4 new joints may also be found in native sulphate of barytes, and it is probable, that by taking proper pains, joints might be discovered parallel to every one of the principal faces of the secondary forms of a mineral.

When only a few joints had been observed in minerals, it was natural to conclude, that the nucleus formed by the planes parallel to those joints was the primitive form; but now the mechanical division has been pushed farther, it must be concluded that the primitive form is unknown.

It might be said, that those natural joints which produce the simplest results, ought to be preferred; but this is entirely arbitrary. In the beryl, some would chuse to take the joints parallel to the lateral faces of the prism, and to the truncations, and thus consider a hexaedral prism as its primitive form: others would take the joints parallel to the faces of the pyramid, and consider a bipyramidal dodecaedron as the nucleus: while a third party, throwing aside the three alternating faces in each pyramid, as has been done in quartz, would substitute a rhomboid for the nucleus. These determinations differ indeed more in appearance than in reality.

In every specimen, it is probable there are invisible joints, which may become manifest in other specimens. Thus, however different may be the more easily denuded joints in calcareous spar and arragonite, these differences may hereafter be found to vanish; especially as some calcareous spars are as hard as arragonite; calcareous stalactiles are found of both species, and there are even specimens which appear to be composed of calcareous spar at one end, and of arragonite at the other.

As the same joints are not always equally neat, observers who examine different specimens, would be led to assign different primitive forms to the same substance. Thus, in the gypsum found near Paris, three natural joints are found parallel to the faces of a straight rhomboidal prism of 113°. On the other hand, some specimens of gypsum, from Mont Blanc, Saltsbourg, &c. have not the joints parallel to the lateral faces of the rhomboidal prism, but in lieu of it, two joints perpendicular to each other,

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so that mineralists who examined these specimens, would conclude that its primitive form was a rectangular parallellopiped.

Hauy allows, that those joints which are the most difficult to hit, may be the most important, and answer to the largest faces of the primitive forms; especially as from the laws of cohesive attraction, it follows, that the more close the contact, the more difficult must be the separation. But experience has now shewn, that minerals have joints not formerly observed; and it may be concluded, that there are still others which are now unknown, from whence it will follow, that the largest faces of the molecules are still unknown.

It must be considered, that hitherto only one person (Hauy) has published a complete system of crystallography: but if it was as much studied as the chemical analysis of minerals, there would be as much diversity of opinion; and it must be remarked, that the smallest difference in the determination of the primitive form is of essential consequence.

Count Bournon is certainly the person, who, next to Hauy, has studied crystallography; and it is remarkable, that he assigns very different primitive forms to the 6 minerals, which he has treated of in as many special memoirs in the Journal des Mines, from what are assigned to them by Hauy, as follows:

Telesie or saphire. A regular hexaedral prism, Hauy: a rhomboid of 86°, Bournon.

Scheelin calcaire, or tungstate of lime. An octaedron of 66° 24', Lucas, from Hauy's communications: an octaedron of 48°, Bournon.

Arragonite. A rhomboidal prism of 116°, Hauy: a rhomboidal prism of 128°, Bournon.

Arseniate of copper. An obtuse rectangular octaedron, Hauy; several different forms, Bournon.

Anhydrous gypsum. A cube, or nearly such, because the joints are of equal neatness in each direction, Hauy: a kind of rectangular parallelopiped, because the joints are not equally neat in each direction, Bournon.

And lastly, Hauy finds a great resemblance between sahlite and pyroxene, while Bournon can not find any.

It is to be remarked that Bournon knew of the determinations made by Hauy; whence it is evident that it cannot be affirmed that mechanical division will always give the same nucleus, without any variation, to two different mineralists. It is even probable that the results would be more frequently different than identical, except a few substances in which the natural joints are very easily denuded, such as calcareous spar, fluor, sulphuretted lead, and muriate of soda. It is however certain, that mechanical division is one of the most certain characters for some minerals; but in

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others it is of less consequence. Hornblende has two joints that cut each other in an angle of 124°, but it cannot be affirmed that another mineral, in which the joints cut each other in an angle of 62 or even 90 is not hornblende: so that mechanical division can not naturally form an essential character of minerals.

Comparative View of the Analcime of Hauy with the Sarcolite of Thompson. By MR. VAUQUELIN.—*Journ. de Phys. Août*, 1807.

Thomson gives the name of sarcolite to this stone, on account of its being of a flesh colour. Hauy judges from its crystalline forms, as also from some external resemblances, and the similar matrix in which it is found, that it is of the same nature as the analcime.

The properties of analcime and sarcolite are however different.

Analcime scratches glass, its specific gravity is 2.244; it loses 8 and a half per cent. by a strong heat. It does not melt by the same heat as the sarcolite, it has a smooth and vitreous fracture.

Sarcolite is scratched by analcime and glass, its spec. grav. is 2.083, it loses 21 per cent. on calcination, it puffs up and then forms a white phosphorescent enamel, it is lamellar.

Analcime contains 58 per cent of silica, 18 of alumine, 2 of lime, 10 of soda, and 8.5 of water; 3.5 per cent. being lost.

Sarcolite found along with analcime, stilbite, zeolite, and chabazie, by Mr. Faujas St. Fond in the porous lava of Montecchio Maggiore, 5 miles from Vicenza, contains 50 per cent. of silica, 20 of alumine, 4.5 of lime, 4.5 of soda mixed with potash, 21 of water, and some traces of iron.

The perfect identity between the crystalline forms of analcime and sarcolite; although other stones which have a much slighter difference in their constituent parts are very different in their crystallization; is very remarkable.

Observations.—It appears from several observations and papers in the Journal de Physique for 1807 and 1808, that much opposition is made even at Paris against the mineralogic or rather crystallographic system of Hauy. The principal argument is founded upon the number of joints which have lately been discovered in minerals; as may be seen by the preceding paper, on Crystallography, in which the author enters at large into the subject.

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On a Fetid Variety of Quartz. By Mr. ALLUAUD, *the Elder.—Journ. de Phys. Août*, 1807.

This variety was found in breaking some quartz at Chanteloube in order to discover some good specimens of arsenical iron which appeared to be imbedded in it.

This quartz is very fetid by percussion, but not by friction, unless rarely, and that it be rubbed by a substance capable of scratching it. The gas is very volatile, has the same scent as stinkstone, and is apparently sulphuretted hydrogen. The quartz has, in general, a glassy fracture, lamellar in some parts and granular in others; it has numerous cracks, is translucid on the edges, and is sometimes transparent. It is generally smoke grey, but of an uneven tint. Before the blowpipe it decrepitates, the gas is dissipated, and it becomes white.

This fetid quartz composes part of a granite that contains emerald; it is found in large blocks, coated with rose red felspar, of which some part is decomposed. These blocks are very large, and lie in one direction in granite; and in a lower bed is found the quartz which contains insulated blocks of mica and emerald. It was originally conceived that the blocks of fetid quartz, which for the most part are very slightly so, formed a vein, but they and the other blocks are similar in their aggregation to the granite that surrounds them.

There are no pyrites in the neighbourhood to furnish sulphuretted hydrogen, and it is not likely to have arisen from the putrefaction of organic bodies, the solution of which has filtred through the cracks, and the disengaged gases penetrated the mass. But the fetid gas was probably combined with the quartz when first deposited, and this seems to strengthen the opinion of Dolomieu, who supposes that sulphuretted hydrogen was an active solvent in the original solution and crystallisation of the globe, and was afterwards volatilized into the highest regions of the atmosphere.

It is rare to find elastic fluids detained in earthy substances at the instant of their concretion, so that it is not surprising that the fluid is not equally distributed through the mass, and that the granite that envelopes the whole is not fetid; but it is more extraordinary why the felspar, emerald, &c. which accompany the fetid quartz, are not themselves fetid.

Mr. Demorange has since found still more fetid quartz near Nantes.

In a note it is said that Mr. Lelievre found in the Isle of Elba a cellular quartz, becoming fetid by rubbing.

No. XIX will be published August 1, 1809.

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RETROSPECT

OF

PHILOSOPHICAL, MECHANICAL, CHEMICAL AND AGRICULTURAL DISCOVERIES.

No. XIX.] *April, May, June.* [1809.

NATURAL PHILOSOPHY, ARTS, AND MANUFACTURES.

*Essays on Electricity. By Mr. * * *.*

1. *On electrical Attractions and Repulsions.—Journ. de Phys. Vol. LXIII.*
2. *Electrical Experiments on Glass considered as a Leyden Phial, and on coated Panes.—Journ. de Phys. Vol. LXIV.*
3. *On the Penetrability of Glass by the electric Fluid.—Journ. de Phys. Vol. LXV.*
4. *On the Oxidation of Metals by the electric Fluid. In a Letter to J. C. Delamétherie.—Journ. de Phys. Vol. LXVII.*

1. *Electrical Attractions and Repulsions.*—This author's experiments on magnetism induced him to doubt the existence of two electric fluids, possessing the power of repulsion with respect to each other; and he conceives that all the phenomena which seem to favour such an opinion may be more simply and naturally accounted for by a double affinity. He, therefore, transcribes the six following experiments, which M. Libes has given, in his *Nouveau Dictionnaire de Physique*, in support of the theory of alternate attraction and repulsion; and explains the phenomena which they exhibit on the principle of affinity only.

Experiments.—1st. When a glass tube has been rendered sensibly electric by rubbing, and a bit of foil, down, or any other light substance let fall upon it, this last is first attracted and then suddenly repelled by the tube. If it be followed with the tube, it will recede rapidly in any given direction; but if it meet with any other conducting substance that is not electrified, it

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immediately returns to the tube, and quickly separates from it again. If it were suspended by a silk thread between the tube and another body of the nature just specified, it would fly alternately from the one to the other.

Attraction only is necessary to account for this circumstance. This writer conceives that it is the attraction which metal is known to possess for the electric matter that causes the foil to approach the tube, to satisfy its affinity for the fluid; and if there be any other substance near it which has also an affinity for the same fluid sufficient to overcome the gravitation of the foil, it will attract and imbibe in its turn the electric fluid, and convey it to the ground. If the affinity of the foil be still sufficiently powerful to impel it again towards the tube, these motions will be alternately repeated, until the force of gravity exceeds the electric attraction. When there is not any conducting substance near to attract the foil, the moisture of the atmosphere performs this office, which also imparts to the down its conducting property, and its affinity for the fluid.

Exper. 2. If a tuft of twisted thread be suspended from the conductor of an electrical machine, and electrified, the threads which were previously twisted together will separate from each other, and their respective distances will become greater as the electricity is more powerful. Here too the aid of repulsion is not deemed necessary in the explanation: the moisture of the air attracts the electricity and separates the threads as far as possible; and the greater the quantity of fluid that each thread has imbibed, the more will the aqueous attraction act upon it, and the distance from the other threads increase.

Exper. 3. If a plate of metal be suspended from the conductor so as to be capable of being electrified by it, and another similar plate placed about two inches below it, containing some small bits of gold leaf; when the upper plate is electrified, the pieces of gold leaf will be attracted by it, and repelled to the lower one alternately: these attractions and *repulsions* continuing as long as the conductor remains electrified. To render this experiment more entertaining, small painted figures may be substituted for the gold leaf, which being alternately attracted and *repelled* by the upper plate, will appear to dance between the two. These little figures are attracted to the upper plate by their affinity for the electric fluid it contains, and towards the lower plate by its affinity for the electricity they have acquired; and no repelling power is necessary to account for their alternate motions.

Exper. 4. A pointed metal rod being attached to a conductor, and the inside of a glass held in both hands presented to it, and afterwards placed over some balls of elder pith laid on a table, they will immediately begin to leap up against its sides, and continue to do so for some time. This experiment is explained upon

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the same principles as those which precede it. The glass becomes charged with the electric fluid, and the balls are urged by the power of affinity to its sides, and attracted by the table in its turn.

Exper. 5. Having suspended a metallic rod by its middle from the conductor of an electrical machine, hang to the former three bells, one from each extremity, and one from the middle. Those from its ends should be suspended by metal chains; the middle one, and two small metal balls between it and the other bells, by silk threads. From the inside of the middle bell a chain must communicate with the floor, or be held in the hand during the experiment. The machine being put in motion, the extreme bells become electrified, and each of the balls is attracted by its corresponding bell, and having struck this, it is immediately *repelled* towards the middle bell: these alternate motions being continued as long as the conductor retains its electrified state. These electric chimes, this author observes, afford the strongest proof of his inferences. The balls are first attracted towards the bells which are saturated with the fluid; the middle bell, which is destitute of any superabundant electricity, and communicates with the ground, attracts the electricity acquired from the other bells by the balls, and with it the balls themselves. This action may be compared with that of an electric kite, which he also conceives to be a strong confirmation of his theory.

Exper. 6. Suspend horizontally, from the conductor of an electrical machine, a ring of strong brass wire, and place a circular plate of metal at a small distance below it. Place a very thin globe of glass on the plate, with one point of its surface in contact with the ring. The whole apparatus being electrified, a double motion will be communicated to the ball or globe of glass; viz. a rotary motion about its own axis, and a circular motion round the ring. If the experiment be made in the dark, the globe will appear luminous in each successive point that touches the ring. This writer considers this experiment as affording a singular confirmation of the inferences he deduced from his own experiments relative to the non-conducting state of glass, which he regards as owing only to the very powerful affinity of composition of its constituent particles; and that when a sufficiently powerful affinity is presented to this substance, it becomes a conductor. The ball of glass is put in motion by the electric action, and being moved in one point, the adjacent points tend by affinity to approach successively towards the centre of activity. "This (he adds) is the natural way in which I explain these phenomena according to my mode of seeing them: perhaps it is by the *idola specus* of Bacon. But, si quid novisti rectius istis, candidus imperti—I seek truth only; this alone guides my pen, and in seeking it I write *commenta*, which time perhaps may efface."

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2. *Electrical Experiments on Glass.*

From a variety of ingenious experiments both of his own and others, this gentleman has been induced to conclude that there is an actual passage for the electric fluid through the pores of glass, when it communicates on one side with the conductor of an electrical machine in motion, and on the other with the ground, by means of conducting bodies; and that in order to obtain this passage, it is not necessary that the glass be coated on both sides, provided it be so at the point of contact with the machine, and touch at a single point some substance which is an imperfect conductor, and has sufficient force to communicate the attraction of the earth through its pores. As it is a fundamental law of chemistry and of physics that motion cannot take place without previous impulse, and therefore without an immediate action on the substance from which the fluid is to be taken, this author thinks that the theory of taking the fluid from the surface of an impenetrable substance, as Dr. Franklin affirms glass to be, is inadmissible. Besides, what substance cannot the igneous matter penetrate? And no person will deny that this matter constitutes a part of the electric fluid.

The various experiments by which this writer conceives the penetrability of glass to be established, were made with the apparatus both in an insulated and uninsulated state, with the electrophorus, &c. And it is inferred from the experiments of M. Lugt, that glass has such an affinity for the fluid as to retain the same quantity of it in an uninsulated as in an insulated state. This is to be accounted for, like all chemical and physical phenomena, by the theory of elective attraction, which these experiments confirm.

One of the most interesting of these experiments is the following: a glass electrophorus was placed on an insulated stand, the lower coating of which was nearly as extensive as the glass; the machine being stopped the moment that the sparks announced a near approximation to a spontaneous discharge, the communication with the ground, in this state, was cut off, and the cap taken from the upper surface, the whole charge adhered to the glass; and on touching it, a prickling sensation, and something like an igneous vapour, was felt. The light being extinguished, it became visible by approaching the edge; but the fluid became absolutely luminous by blowing slightly on the surface; when a wave of fire traversed the glass, in order to unite with the fluid accumulated between the glass and the metallic coating on the opposite side. It was particularly remarkable that two colours might be distinguished in the fluid, the lower one being whiter and more vivid than the upper one. This phenomenon is also observable if the communication be uninterrupted. This experiment, its author says, gives rise to the question, whether all the ingredients pass through the substance of the glass, or whether

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the difference of action is to be attributed to the state of the glass only? He believes it is this modification which the electric matter seems to undergo, that constitutes the opposite states, which different philosophers endeavour to explain in different ways, according to their mode of viewing them. Does not this experiment likewise prove that the reciprocal attraction between the surfaces of the glass retains the fluid on its upper side, notwithstanding that the cap is taken off? This fact the writer conceives he has verified by experiments on phials filled with water, and poured from one to another when electrified, both in an insulated and uninsulated state.

In the concluding reflections in this paper, the author observes, "I do not offer these views to the natural philosopher with a design of creating a new theory, but as an inquiry whether or not the igneous phenomena of magnetism, galvanism, electricity, and detonations, be subordinate to the general law of affinities." The experiments with which this department of science has been enriched by *Libes* and *Ermann* support the hypothesis that there is only one kind of igneous matter, which constitutes light, the magnetic, galvanic, and electric fluids, &c. and which is modified in them by different ingredients. Ermann's experiments prove that the flame of alcohol contains ingredients that differ from those which constitute that of sulphur or phosphorus. Do not the gasses evolved from different metals by the same acids, and the various colours of artificial fire-works, demonstrate that the atmospheric caloric, united with the latent or combustible ingredients, carries off various particles, the number of which will ever remain unknown to us? Of the nature of carbon, nitrogen, hydrogen, and oxygen, notwithstanding their abundance, we know nothing. Are they simple, or are they compound substances? How many varieties do these four bases afford, merely by the different proportions in which they are combined? And why does the new inflammable mixture, which induced Proust to abandon his experiments, appear still more terrible than fulminating silver?

3. *On the Penetrability of Glass by the electric Fluid.*

Many of this author's experiments having proved that the electric fluid, in its penetration of glass, commences at the centre of contact of the two coatings, and extends its rays in proportion to the quantity of matter which passes at once, or to the continuity of the passage; he conceived that it departed from a phial to traverse the thickness of a plate of glass upon which it was placed; and that this circumstance would afford a new proof of several of his previous inferences. He then describes some experiments made with a view to ascertain this point; but as they refer to plates, and are little susceptible of abridgment, we can only refer such of our readers as feel interested in these in-

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quiries to the original essay, and state in a few words his chief inferences or results.

He observes—"I can only consider the substance of glass as being full of capillary pores, which intersect each other in all directions, and retain a part of the fluid in the passage. All these experiments appear to confirm more and more that all the electric phenomena are subordinate to the elective theory."

4. *On the Oxidation of Metals by the electric Fluid.*

By exposing silver wires immersed in tubes of water to the action of a current of electricity for the space of four hours successively, they became covered with a kind of gray oxide; and the effects were identical, whether the wires were immersed in distilled or pump water. After having varied the experiments, and obtained similar results, he gives the following directions for oxidating two wires at once. Range two silver wires parallel to each other in a tube filled with water; invert the tube, and immerse it in the water of a phial. Instead of bending the upper extremity of the wire near the tube, let it be lengthened eight or ten inches, and a small silver ball soldered to it at the height which corresponds to the exciter. Then at each detonation the positive fluid must enter by the upper wire, while at each charge it passes by the inferior wire. In half an hour's operation the surfaces of the wires will be changed black, and four hours will reduce them to a state of oxidation. If a large phial be used from which sparks emanate, the tube will burst in a short time: this circumstance renders a small phial, which gives about ten detonations per minute, preferable.

Observations.—Those who are conversant with the elegant and satisfactory theory by which the followers of Franklin have explained the phenomena of electricity, and with the experiments by which that theory is supported, will not require many remarks to assist them in forming an opinion of these papers, which are evidently the work of a person very little acquainted with the science which he is attempting to improve. It will only be necessary for us to advert to the first of the experiments which he copies from Libes. It is well known that this experiment, like almost all others in electricity, succeeds best when the air is driest: how, therefore, is it possible that its result can be derived from the presence of moisture only? Our author does not deny that the light body, "if it be followed with the tube, will recede rapidly" before it; and how can the approach of the tube increase the quantity of the moisture on the other side, or its attraction for the light body? If the body attracted the air, the air would approach to it, and become condensed, and there would then be an equilibrium, which would be wholly independent of the presence or absence of the tube. The attempt to explain each of the other experiments is equally superficial and

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unsatisfactory; and if the author had been acquainted with the facts relating to the state of bodies electrified by induction, he would have seen that these are wholly irreconcilable with his opinion. One class of these facts comprehends the phenomena of charged glass, respecting which our author has remarked nothing new: the luminous appearance produced by blowing in it must be owing to the electrification of the moisture contained in the breath. The experiments respecting the oxidation of metals seem to have no direct connexion with the theory of electricity.

An Essay on Electricity, or a Series of Considerations on the State of a Bed of insulated Bodies interposed between two Surfaces charged with contrary Kinds of Electricity. By M. A. Avogadro.—*Journ. de Phys. August* 1807.

M. Avogadro has been conducted, by considerations on this subject, to the conclusion, that an insulated bed interposed between two surfaces charged with contrary kinds of electricity, that is to say, a bed electrically charged, offers a series of elementary beds or films similarly charged. He observes, we have, therefore, a particular modification which obtains in all the molecules of a bed of air interposed between two bodies which attract each other by contrary electricities, since this bed of air is necessarily charged. It seems difficult not to admit that this modification is the means by which the attraction operates, as it does not appear proper to admit any other force which acts at a distance between two contrary electricities; and thus we must suppose that in this case it is the action of chemical affinity which operates by a series of actions at insensible distances, as appears to take place in the phenomena of refraction, and of capillary attraction. It is very probable that if we could form an idea of the physical nature of this electric charge, it would conduct us to the manner in which electrical attraction is exercised, and consequently to that in which repulsion acts between two bodies charged with the same kind of electricity, which is to be attributed to a similar modification, but acting in an inverse manner. It is an examination of this idea which M. Avogadro has proposed for the subject of this Essay.

Little can be conceived of electric phenomena without the intervention of some particular fluid, expanded in all bodies, to which the modifications must be referred. With respect to this fluid there are two hypotheses; the one admitting one fluid only, the other supposes two different fluids. In attempting to ascertain the modification which constitutes the electric charge, our author observes, it is necessary to determine for one of these hypotheses. If we consider the electric forces as exercised by

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actions at a sensible distance, we are led, in the case of a single fluid, to this result, that the molecules of all bodies considered as deprived of the electric fluid, mutually repel each other. In the hypotheses of two fluids we admit a repulsion at a distance between the molecules of a particular fluid, which, by its excess, constitutes positive electricity. But M. Avogadro observes, that the reason for preferring the hypothesis of two fluids does not obtain when we attribute attraction and repulsion to analogous modifications in the air interposed between two electrified bodies; for then it is the fluid of the air differently modified by the two similar or contrary electricities between which it is comprized, that is the agent of attraction or of repulsion; and in the case of similar electricities, we may conceive that a modification proper to occasion repulsion, may be equally produced by an excess, or by a defect of the fluid resident upon the surfaces of the two electrified bodies; that is, either by two positive or two negative electricities. As the supposition of two fluids would only give to the phenomena a double explanation, by a double cause acting in the same manner, this author thinks it is clear that it is towards a single fluid that we ought to direct our researches, in order to introduce into them a proper degree of simplicity.

As the charge affects all the molecules of the bed, it is a chemical modification. But a modification of this kind can consist only in one of these two things; either in the combination or separation of an absolute quantity of some substance which did not before exist, or which ceases to exist in the charged system; or in the different arrangement of a substance existing in all bodies susceptible of being charged. This author regards the former of these suppositions as being inadmissible, and therefore adopts the latter: and, whether the electrified surfaces be in contact with each other, or separated by a bed of air, he conceives that the electrical charge consists in the simple displacing of some substance contained in the bodies which receive it, or in the different arrangement of this substance between their molecules; and this is that substance which we call the electric fluid.

M. Avogadro then examines the most simple case of what he denominates charged system, viz. that in which the surfaces of the two electrified substances are in actual contact. This he considers as the primitive alteration of the natural state, as it is that which takes place at the moment the fluid is disengaged by means of friction, and from which all the other electrical phenomena arise. By considering the two bodies under this circumstance, he thinks it is evident, that, as the interior faces of the two substances in contact only ought to undergo a change, the displacing of the fluid can only consist in the transposition of it from the superficial molecules of one body to those of the other; and that this transposition, whatever may be its cause, is

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made without any resistance; that an equilibrium only obtains between the two surfaces, and that when the fluid is once displaced, it exercises no tendency to return to its former state as long as the bodies remain in contact. From this he concludes, that this transposition is not made by a common chemical combination, occasioned by the superior affinity of the molecules of one of the substances over that exercised by the molecules of the other, but by a chemical combination of a particular nature, of which we have hitherto had no precise knowledge. It is the contact of these two surfaces which, suspending the laws of common affinity, permits this combination; but the contact can only influence the hemisphere of the superficial molecules which are brought by it to touch each other. It is therefore only on these hemispheres that the diminution on one side and the accession on the other of a certain portion of the substance, the transposition of which gives rise to electricity, can take place. We are therefore conducted by the electric phenomena to a chemical fact of which we had no previous idea: "there may exist upon one of the hemispheres of each elementary molecule of any body, a surcharge of a rare substance, which otherwise tends to distribute itself between the molecules by reason of their mutual affinity, when the hemisphere upon which the matter rests belongs to a surface in contact with another, each molecule of which has, with respect to the hemisphere situated on the face of the former, an equal defect of the same substance relative to the portion which its natural affinity requires; so that the total quantity of the substance in question between the two hemispheres may be the same as that required by their united affinities, with respect to the surrounding molecules."

The new kind of combination, which is here called *electric*, being once admitted, it appears that we may conceive it in any known substance or fluid whatever, of which the elementary molecules of two bodies in contact may be considered as the base; and this opens a way of explaining, by different fluids, modified in this manner, the phenomena of electricity, magnetism, &c. which are so analogous to each other. These facts conduct to something sufficiently precise upon the nature of electricity, considered with respect to the two electrified surfaces in contact with each other, as stated above. It will consist in a surcharge of the fluid upon the hemispheres of the molecules which constitute one of the two electrified surfaces, and a corresponding defect of the fluid upon the hemispheres belonging to the molecules of the other electrified surface, in contact with the former.

The next inquiry relates to the case in which the two electric bodies are separated from each other, or have a bed of air interposed between them. In this separated state, the excess of the fluid in the hemispheres of one of the surfaces ceases to find an

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immediate place in the defect of the hemispheres of the other surface. With respect to the mode of communication between these two surfaces, Mr. A. observes, it may be conceived that the kind of compression which ought to result from an excess of fluid attached to the hemisphere of a molecule of the coating or electrified surface, which is applied to one of the hemispheres of a molecule of the charged body or separating substance, and that the simultaneous cessation of the natural pressure upon the fluid of the other hemisphere of the same molecule, occasioned by an electrical defect in the molecule of the other coating or surface which will be in contact with that hemisphere, ought to displace the whole atmospheric body of the fluid of this molecule thus taken between an excess and a defect, so that this atmosphere is carried more towards one side of the molecule than towards the other; thus forming, as it were, a place for the excess of the contiguous molecule, and occupying that left by the defect of that contiguous to the other side. What has been said respecting a single molecule situated between the hemispheres of two molecules charged with contrary kinds of electricity, is equally true of a series of molecules placed between the same hemispheres, and therefore of all those which are in the same state, and which constitute the thickness of the separating substance. It is this eccentricity of all the fluid atmospheres of the molecules of the charged body, an eccentricity from which results a kind of excess on one side and of defect on the other, which constitutes the electric charge. We may conceive that this state of each molecule is a forced state, contrary to the law of attraction, and from which it endeavours to free itself with a certain force, greater or less, according to the degree of this eccentricity, which depends upon the quantity of the electric fluid accumulated in the coatings. Hence results a tendency in these electricities of the coatings to dissipate themselves, so much stronger, with an equal density, as the number of the molecules put into this state by each molecule of the coatings is greater, that is, as the thickness of the charged body is more considerable. This will therefore be the cause of the *electric intensity*, which was nothing in the state of contact of the two electrified surfaces, because there was then no fluid atmosphere displaced.

M. Avogadro then examines the mode in which the electrical discharge is effected: this he supposes may take place in two different ways. The one will be the mutual destruction of the two equal and contrary electricities of the surfaces by communication. The other consists in a transposition of the excess or positive electricity accumulated on one of the surfaces by means of the intervening substance, to the defect or negative electricity of the other surface, in order to restore the equilibrium of quiescent state.

Observations.—There is some ingenuity in M. Avogadro's mode of considering the subject; but theories such as he has ad-

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vanced ought rather to be supported by experiment than by reasoning. It has long been ascertained, that the presence of air makes little or no difference in the repulsive force of electricity, as indicated by the separation of the balls of an electrometer: the separation taking place nearly, if not altogether, to the same extent in the vacuum of an air-pump as in the open air, which could not happen if the interposition of the particles of air were essential to the operation of the forces concerned: and M. Avogadro must endeavour to supersede the experiments of Stephen Gray and of other later electricians before he can establish his own opinions.

On the Influence of Electricity upon Flame. By M. Leopold Vacca.—*Journ. de Physique, Sept.* 1807.

If the water which is discharged from a small tube drop by drop be electrified, the dropping is changed into a stream, and the same effect applied to a stream increases its velocity. The cause of these effects is well known: the component particles of the water being all charged with the same kind of electricity, mutually repel each other. From this we have only to conceive flame to consist of an assemblage of extremely subtile molecules, which repulse each other in a similar manner when charged with the same kind of electricity.

In order to prove this, M. Vacca took a small metallic vessel, filled with spirit of wine, and having insulated it, and formed a communication between it and an electrifying machine by means of a metallic chain, he inflamed the spirit of wine without putting the machine in motion, and observed the bulk and figure of the flame. He then caused the machine to act, and observed that it caused a very considerable diminution in the volume of the flame. When the action of the machine was suspended, the flame assumed its former dimensions. The experiment often repeated always gave the same results.

It is known that electricity passing out of a body into the atmosphere repels it nearly in the same manner as the explosion of powder repels a cannon when it is fired. By means of points in a star of metal, moveable on a pivot, the electricity imparted to the star is dispersed into the atmosphere, and the star is caused to turn on the pivot in a direction contrary to that of the points. It is also known that flame is a body highly proper for dispersing electricity. Therefore, if from all the points which compose the surface of flame electricity escape, these points ought all to be repulsed within the flame, and consequently the flame will be compressed, and have its volume diminished.

Observations.—It must not be understood, that the discharge of

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the electric fluid from a point repels that point by the mere inertia of the fluid, in the same manner as a stream of water issuing from the side of a vessel causes that vessel, if moveable, to recede in a contrary direction; for the electric fluid, whatever may be its nature, appears to possess no perceptible momentum. The fact is, that the particles of air in contact with the point receive its electricity, and are repulsed by it; a succession of fresh particles is derived from the lateral streams which flow in all directions towards the point, and there, becoming electrified, contribute in their turn to the repulsive action. But since flame acts like a point with respect to the diffusion of electricity, it is very natural that the streams of air which rush towards it horizontally should compress it a little in that direction, while the repulsion of the particles of air already electrified, which are situated principally above it, tend to contract its length. Further than this the experiment of M. Vacca appears to be of no particular importance.

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1. *On the Motion of floating Bodies. By Mr.* G. Orr.—*Phil. Mag. No.* 131.
2. *On the Motion of floating Bodies in running Water. By* Peter Barlow, *Esq. of the Royal Military Academy, Woolwich.*—*Phil. Mag. No.* 132.

1. Mr. Orr's object in this paper is to account for the circumstance of loaded barges gliding faster than the current, or than unloaded barges; and the heavier end of a pole always swimming first. These two problems, he observes, may be solved in the same way—they are the result of gravity. The barge, he considers as moving by the force of gravity down an inclined plane, and that therefore its velocity will be in proportion to its weight, or to the quantity of matter moving together; and that, abstracting resistance, its velocity will be increased in a certain ratio as it proceeds. This case he regards as exactly analogous to that of loaded and unloaded carriages going down a hill; or to that of a cannon ball and a round piece of wood of the same dimensions rolling down an inclined plane: "the cannon-ball will roll quickest, because it contains more matter; the heavy end of a beam or stick proceeds down the stream first, because it observes exactly the same laws of matter and motion."

2. The first part of Mr. Barlow's letter is occupied in correcting the errors into which Mr. Orr has fallen in the first of these articles. He observes that Mr. O. does not seem to comprehend the nature of what he terms the "laws of matter and motion." All bodies, whatever be their magnitudes and densities, when affected only by the force of gravity, will descend through equal spaces in equal times; and when this is not the case, it originates from some op-

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posing force. Thus, each of the globes, in the case proposed by Mr. Orr, in descending down the inclined plane, experiences the same resistance from the atmosphere, but being of different densities, they oppose to it different forces, and consequently descend in different times, and with different velocities. "We see, therefore, that instead of gravity being the cause of the different rates of descent of the two bodies, the circumstance must be attributed to its laws being counteracted." Mr. Orr is not less mistaken in considering this as a parallel case to two bodies floating in running water; for, in the former case, the bodies move through a medium which is perfectly at rest; and in the latter, the medium itself is in motion, and is the cause of the motion of the bodies."

Mr B. then offers his own conjectures by way of accounting for the same circumstance. He supposes a beam loaded at one end, at first moving parallel to itself, in which case, both ends will evidently move with the same velocity, but the loaded end will acquire the greater momentum by the motion; and consequently oppose any resistance, which the whole beam may meet with in its passage, with greater effect: whence the comparative velocity of this end will be increased, and the beam will gradually deviate more and more from its parallel motion, until its direction coincide with that of the stream. The same reasoning is also applicable to barges in similar circumstances. The resistance may arise from the body coming in contact with any particles of water moving either in contrary or different directions with any that are at rest, or with such as move in the same direction, but with a less velocity.

Observations.—To such of our readers as are at all acquainted with philosophical subjects we conceive it would be entirely superfluous for us to state which of these gentlemen's principles of solution are correct, as far as they go, and according to the suppositions upon which they rest. These suppositions appear to be, that both ends of the beam or stick are of the same magnitude, that it partakes of the same velocity as the water at the moment of its immersion, and that it maintains a horizontal position. *We*, however, regard these as circumstances which do not actually take place in experiments of this nature; and *if* so, neither of these solutions can be deemed of any value. This problem is of a different nature, and requires a much greater extent of investigation than either of these gentlemen appears to have conceived. It naturally involves different cases, in which either the heavier or the lighter end of the beam may go first, according to the number and extent of the variations from the circumstances they have assumed. We shall be glad to see each of these cases completely investigated in Captain Burney's paper; and as it is highly probable that its appearance in the Philosophical Transactions will shortly

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recal our attention to the subject, we shall decline entering at large into it at present; and shall content ourselves with briefly observing, that the acceleration of the motion of a solid floating on the surface of a river, must depend on a circumstance not altogether unnoticed by writers on hydraulics; which is, that the mutual friction of a solid and a fluid is less than the friction between two strata of the fluid; so that, although the barge is no heavier than the quantity of water which it displaces, the weight of which would only carry it along the slightly inclined surface of the river with the common velocity, overcoming the resistance occasioned by the excess of the velocity of the superficial above the deeper parts of the water; yet when this resistance is diminished by the substitution of a solid for a fluid, the solid is accelerated until its velocity becomes sufficiently great to generate a resistance equivalent to its relative weight, that is, to its tendency to descend along the slightly inclined surface of the fluid.

Description of a new Fence made of tort elastic Wire, which becomes invisible at a comparatively short Distance, calculated for Pleasure-grounds. By HENRY HOWELL, *Esq.—Phil. Mag. No.* 132.

THE basis of this fence is elastic iron wire of the thickness of a common quill, inserted horizontally through upright stanchions; the intervals between which are about seven feet, and that between the wires about nine inches. The wires are fastened to two principal stanchions at the extremities of the fence, and pass freely through holes in the intermediate ones. When the fence is very extensive, the principal stanchions are relieved by others at convenient intervals. The elasticity of the wire permits it to bend, and when the force is removed, it causes it to recoil and vibrate until it rest in its original straight position. A fence of this kind has been found sufficient to exclude cattle when about three feet six inches high; and when this height is increased about two feet more, it is applicable to deer-parks. When it is designed to confine any thing of a smaller nature, perpendicular wires of a less size may be interwoven with the lower horizontal wires, at short distances from each other. The distance at which this fence becomes invisible is said to be about seventy yards.

Observations.—The kind of wire above recommended for fences of this nature is said to be manufactured and applied, upon principles discovered by Mr. James Pilton of Chelsea; who has been employed to erect several of them both in the Royal pleasure-grounds at Frogmore, and in the parks of noblemen and gentlemen in other parts of the country; and which have been found fully to answer the intended purpose. The chief expense will

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evidently arise from the value of the iron stanchions and the blocks of stone necessary for fixing them in their respective places with sufficient firmness, and which will require to be very considerable, as they can derive so little support from the wires. This expense, however (of which Mr. H. has not given any estimate), will be deemed an object of small consequence where required for the purpose of guarding pleasure-grounds, ornamented walks, &c. without interrupting the prospect. Where utility, independent of ornament, is the chief object to be attained by the erection of fences, we apprehend that one of this kind might be used with advantage, by adopting oak posts instead of iron stanchions, as the expense would be less than that of most other kinds, and the durability certainly not inferior; especially when the fence was well painted, which would cost but little. This advantage we think would be considerable in many upland districts, where quick fences are rare and wood is scarce. In these exposed situations, the injury which other fences generally sustain from high winds would be avoided in this; and when the fence is required between sheep-walks and tillage, as is frequently the case, a less height than that mentioned above would be sufficient; and from the toughness the operation of drawing the wire is known to impart to the metal, and the experiments which have proved iron wire to be much stronger than it is generally thought to be, we think that in such cases smaller wire might be employed than that specified by Mr. Howell. Might not small painted wires, inserted through stakes, driven into the ground in the usual manner, be also adopted with advantage for sheep-folds, instead of netting, &c. now used for that purpose?

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1. *Remarks on M. Burckhardt's Contrivance for shortening reflecting Telescopes; with a new Method of making refracting Telescopes with a Tube only one Third of the focal Length of the Object-glass. By* David Brewster, *LL.D. F.R.S. and F.A.S. Edin.—Phil. Mag. No.* 182.
2. *On the Fibres used in Micrometers: with an Account of a Method of removing the Error arising from the Inflection of Light, by employing hollow Fibres of Glass. By* David Brewster, *LL.D.—Phil. Mag. No.* 133.

1. M. Burckhardt's new telescope, as described in the *Connaissance des Tems* for 1809, is of an intermediate nature between the Gregorian and Newtonian telescopes; and requires a tube only half as long as other telescopes of the same focal length. The large concave speculum is perforated as in the Gregorian telescope, the diameter of the aperture being half that of the speculum. The parallel rays, which after reflection would have

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formed an image at the focus, are intercepted by a plane speculum of the same diameter as the aperture, placed perpendicular to the axis of the telescope, and are thence reflected back to a point at the centre of the aperture, where the image is formed and viewed with an eye-glass. By giving the plane speculum a small inclination to the axis of the telescope, the image may be formed above the large speculum, and no perforation in its centre will be necessary.

Dr. Brewster remarks that the merit of this invention does not exclusively belong to M. Burckhardt; as the same principle was employed more than six years ago in the construction of a reflecting telescope, by Mr. Paterson, of Edinburgh, and instruments of this kind were advertised for sale in that city. He also constructed a small one several years since; from which he concluded, that the disadvantages necessarily attending the shortening of the tube, would more than counterbalance the convenience that would be derived from it. M. Burckhardt, however, is of a different opinion; and as his views have been seconded by the French Board of Longitude, and taken up by their astronomers, Dr. B. has given an account of two methods of effecting the same purpose, which have occurred to him. He observes, that if for the plane speculum a convex one be substituted, so as to form the image at the centre of the large speculum, as in M. Burckhardt's, the telescope will become cassegrainian, and the length of the tube be reduced to about half the focal length of the convex speculum. This construction, Dr. B. regards as preferable to that of Burckhardt, because a correct figure is more easily given to a convex than to a plane speculum, and the errors in one speculum of the former kind frequently correct those in another.

If the circumstance of shortening the tubes of reflecting telescopes be productive of any real advantages, Dr. B. thinks it a matter of importance that those of refracting telescopes should possess similar properties; and he describes the following contrivance for effecting this purpose, by which the tube is reduced to one third of the focal length of the object-glass. At a distance from the object-glass equal to one third of its focal length, is placed a plane speculum, the diameter of which is two thirds of that of the object-glass. By giving this plane speculum a small inclination, the rays are reflected to a second plane speculum, placed in the same plane with the object-glass, and equal to one third of its diameter; from this they are again reflected to a focus "where the image is formed and magnified by the eye-piece. In this construction, the only disadvantage is the loss of light occasioned by two reflections; but this may be obviated by increasing the aperture of the object-glass, and is by no means such a serious evil as that which arises in M. Burckhardt's contrivance, from the loss of such a large central portion of the speculum."

2. After adverting to the difficulty of procuring fibres sufficiently

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fine and elastic for these delicate instruments, Dr. Brewster remarks, that a particular part of the spider's web, fibres of raw silk, and silver wire, have all been used for this purpose. The ingenious Mr. Troughton found the first of these so fine, opake, and elastic, as to answer all the purposes of practical astronomy; but as the stretcher or long line which supports the web is the only proper part on account of its opacity, the difficulty of obtaining it caused the other two to be frequently used: all these, however, are "liable to the error arising from the inflection of light, which renders it impossible to ascertain the exact contact between the fibre and the luminous body," a disadvantage which has always been considered as inseparable from the wire micrometer. This disadvantage, Dr. B. has been able to remove by employing a fibre made of glass; which may be drawn to any degree of fineness without losing its elasticity and opakeness, and can always be procured with facility.

Of these, he observes, "It is evident that this vitreous fibre, when drawn from a hollow glass tube, will also be of a tubular structure, and that its interior diameter may always be regulated by the inner diameter of the original tube. When the fibre is formed and stretched across the diaphragm of the eye-piece of a telescope, it will appear perfectly opake, with a delicate line of light extending along its axis. This central transparency arises from the transmission of the incident light through the axis of the hollow tube; and since this tube can be made of any calibre, we can also increase or diminish the diameter of the luminous streak. In a micrometer which I have fitted up in this way, the glass fibres are about the 1200th part of an inch in diameter; and the fringe of light which stretches across their axes is distinctly visible, though it does not exceed the 3000th part of an inch.

"In using these fibres for measuring the angle subtended by two luminous points, whether they be two stars, or the opposite extremities of a luminous disc, we may, as has been done hitherto, separate the fibres till the luminous points are in contact with their interior surfaces; but in order to avoid the error arising from inflection, I would propose to separate the fibres till the rays of light issuing from the luminous points dart through the transparent axes of the fibres. The rays thus transmitted evidently suffer no inflection in passing through the fibre to the eye; and besides this advantage, we have the benefit of a delicate line about one third of the diameter of the fibre itself."

Observations.—We have before had occasion to commend Dr. Brewster's diligence and skill in the improvement and simplification of optical instruments (see our vols. ii. iii. and iv.), and we consider these papers as highly worthy the attention of opticians and astronomers. The plan for shortening a telescope is, however, far less essential at present than before the im-

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vention of achromatic glasses; and, in our opinion, the introduction of two specula must in most cases be likely to cause imperfections in the focus, which would more than counterbalance any convenience that could be derived from the diminution of the length. With respect to the employment of a tubular fibre for a micrometer, we apprehend that it would by no means possess all the advantages which the ingenious inventor expects from it: for it is easy to observe, by holding the tube of a thermometer or barometer before a candle, that the transit of the candle behind it produces no immediate effect on the brilliancy of the bore, and cannot therefore be ascertained by means of the "rays darting through the transparent axes."

Remarks on Hygrometry and the Hygrometer of J. Berzelius. In a Letter from Mr. J. Gough to Mr. Tilloch.—Phil. Mag. No. 131,

Mr. Gough commences this Letter by observing that few additions could be made to the apparatus of a meteorologist that would be more acceptable than a cheap and correct hygrometer; that the various attempts that have been made to improve this instrument have generally increased its sensible effects, instead of diminishing them, which he conceives, with the celebrated Dr. Franklin, would be a more effectual way of attaining the desired objects with ease and certainty. Mr. Gough states, that he has for some time entertained an opinion that a very simple instrument of easy construction would answer the purposes of hygrometry; and a few experiments made with one of this nature, he says, "were far from discouraging the hope." The following description will afford an idea of this instrument, as well as of the mode of constructing and using it.

Having taken a scale of brass or other suitable material, upon which a right line of nine or ten inches in length is drawn and divided into one hundred equal parts; take a square rod of wood, cut across the grain, four or five inches long and about a quarter of an inch square, and draw on one of its surfaces a longitudinal coloured line. Suspend the rod, thus prepared, for several days in a close vessel, containing a quantity of either quick lime or dry potash, by which means the moisture will be extracted from the wood, and it will be brought to a state of great dryness. Then apply one end of the line on the rod perpendicularly to the beginning of the line upon the brass scale, and mark the place of the opposite extremity of this coloured line. Suspend the rod again for an equal number of days, in a similar manner, in a vessel containing water, in order that the wood may become saturated with moisture; then apply it, as before, to the other end of the scale, marking the place of its other extremity,

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as in the first case. Then join these two marks, and the hygrometer is completed. When an experiment is to be made with this hygrometer, apply the rod so that its extremities may coincide with the two lines, and observe the degree pointed out by that end of the coloured line which is applied to the scale, which will denote the state of the hygrometer. Mr. G. observes that hygrometers of this kind have "the recommendation of simplicity; and a common artist might construct a number of them with little trouble and at little expense." The rods in his experiments were made of old dry oak. Dr. Franklin recommended slips of mahogany for the purposes of hygrometry.

This ingenious writer then remarks that he should have deemed this communication too trifling for public notice, if it had not been for the account of Mr. J. Berzelius's hygrometer (which has been noticed at page 36 of our last number). Mr. Gough objects to his hygrometer as being constructed upon the supposition that Mr. Dalton's theory of mixed gases is consistent with the phenomena of meteorology; and conceives that Mr. Berzelius has gone too far when he says that Mr. Dalton has proved the water of the atmosphere to be independent of the air, as this proposition is not demonstrated in any part of this latter gentleman's works. He also deems an implicit answer to this question absolutely necessary to establish Mr. B.'s hygrometer upon sure principles. Besides these remarks, which relate to the hypothesis which suggested the hygrometer in question, Mr. G. urges other objections against the correctness of the instrument itself; and draws the following inferences from what he advances on this head: "1st, That glass and other substances rob the atmosphere of water by an attractive force; and that a loss of temperature does nothing more than prepare the air to part with its moisture more abundantly: 2dly, That the attraction of the hygrometer is resisted by a similar, but opposite power, residing in the atmosphere itself: 3dly, That the phenomena of the instrument are to be explained by the mutual re-action of these contrary forces."

Mr. Gough then presses some further objections; and as these relate chiefly to the interesting subject of meteorology in general, respecting which so much yet remains to be done, we shall present them in his own words: "There is one circumstance which renders the hygrometer of little or no use to the advocates of the new hypothesis: for, if I understand them rightly, a barometer will supply the place of the other instrument, in their opinion. The author of this system maintains the joint pressure of the permanent gases to be 29.56 inches of mercury at all times and places, I suppose on the level of the sea. This being admitted, it will follow, that the same joint pressure may be found for any height; consequently, if this given quantity be

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subtracted from an observed height of a barometer, the remainder will express the expansive force of the aqueous atmosphere for the time and place. If this be what they mean, it must convince every meteorologist that their opinion cannot explain the changes that frequently take place in the weight of the atmosphere. For instance, if the hypothesis be true, the force of the aqueous atmosphere ought to be greatest in summer, and the barometer to be highest at the same season;—but this is not the case. On the contrary, the maximum of elevation commonly happens after the winter solstice, and the minimum too; for the barometer frequently rises, during January, to 30.6, or higher; *i. e.* the force of the atmospherical vapour is equivalent to one inch of mercury, or more. The preceding conclusion cannot be reconciled both to observation and hypothesis; for the thermometer very seldom reaches 48°, at least in Westmoreland, during the month of January; consequently the force of the aqueous atmosphere is never equal to $\frac{4}{10}$ths of an inch of mercury in this county at that season; and the hypothesis seems to confine the range of the barometer to the limits 29.56 and 29.96, which is inconsistent with observation."

Observations.—Much discussion relative to the principles of hygrometry has taken place; but whether these principles ought to be regarded as clearly established, we must leave our readers to decide for themselves, by referring them to our 2d volume, where the leading arguments on the subject have been noticed. We are inclined to welcome any invention or improvement which may have a tendency either to contribute to the accuracy of hygrometrical results, or increase the facility of attaining them; and we still think that well-conducted experiments of this nature would neither be devoid of utility nor interest, as they would either correct or confirm what had previously been done. But we cannot consider either Mr. Gough's inventions or his reasonings as contributing much to the improvement of hygrometry. From the magnitude of his rod, it can only be calculated to shew the mean state of the atmosphere on an average of several hours, and perhaps days, before the time of observation. It must be confessed that some of the circumstances mentioned by Mr. Gough, as attending the deposition of moisture, require somewhat more attention than they have usually received: but if we at all understand Mr. Dalton's principles, Mr. Gough has so much misapprehended them, that it would be too tedious to point out all his mistakes.

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Report of a Manuscript Work of M. André, formerly known by the Name of P. Chrysologue de Gy, entitled, A Theory of the actual Surface of the Earth. By MM. HAUY, LEVIERRE, *and* CUVIER. *Read to the Class of Mathematical and Physical Sciences in the National Institute.—Trans. Inst.* 1808.

THIS Committee commence their Report with some general observations on the manner in which subjects of this nature ought to be considered, and geological researches conducted. They observe that the natural history of unorganized bodies, which constitute the great mass of materials, is divided into two principal branches. In one of these each distinct substance is examined separately, and its rank in the general system ascertained by its physical and chemical properties. This part of geological investigation is termed *mineralogy*: it has long been successfully cultivated by men of science, and has now attained a considerable degree of precision. The other branch of this history of minerals embraces the relative positions of their different species, and of those masses which are composed of their various combinations. It is this branch of the subject that enables us to ascertain the order and extent of the different strata which constitute the exterior part of the globe we inhabit; and which is known by the appellation of *geology, geognosy, or physical geography*, according to the extent or profundity of its researches. In order to extend and improve so interesting and useful a science, and direct its cultivators towards real and attainable objects, they conceive that the utmost encouragement is due to those who state positive facts, and maintain the most rigid silence relative to preceding systems.

Some remarks are then offered on the importance and utility of this science whenever it shall attain any considerable degree of accuracy, and a brief abstract of its history is given. These three learned reporters have then thought it consistent with their duty to present some of the principal objects which it appeared to them necessary to study with the closest attention before geology could become a science of facts, or any attempt could be made, with the least hope of success, to ascertain the causes which have reduced our globe to the state in which we observe it. These are—

1st, To ascertain whether or not the division of great chains of mountains into one middle and two lateral branches, as observed by Pallas, and developed by De Luc, be invariable; and to examine, as Ramond has done on the Pyrenees, the causes by which they are sometimes concealed.

2d, To examine likewise whether or not there be any certainty or uniformity in the succession of secondary strata—whether one

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particular kind of stone be always below another, and *vice versa*.

3d, To examine the fossils in a similar manner, to determine the species which make their appearance first, and those which succeed them; to discover if these two kinds are never mixed together; if there be any alternations in their appearance; the first being found a second time, while the second has totally disappeared.

4th, To form a more minute and accurate comparison between the fossil and the living species than has yet been done; and to ascertain if any relation subsist between the antiquity of the beds, and the similarity or dissimilarity of the fossils with living beings.

5th, To determine if any uniform relation of climate subsist between fossil substances and those animated beings with which they have the greatest resemblance: for example, whether they have emigrated from the north towards the south, from east to west; or whether there have been any mixture or irradiations.

6th, To ascertain those fossils which have lived in the places where they are found, and those which have been transported thither; and if, in this respect, any uniform laws can be perceived with regard to the strata, species, or climates.

7th, To follow these different strata with the most minute attention throughout their whole extent, whatever may be their windings, inclinations, or ruptures; and also to determine what countries belong to the same formation, and which have been separately formed.

8th, To follow the different beds through their varied inclinations, in order to ascertain the relation between the greater or less constancy of their horizontal position, antiquity, and nature.

9th, To discover those vallies in which the saliant and reentering angles correspond, and those in which they differ; also those in which the strata are the same on both sides, and those in which they are not, with a view of ascertaining what relation there is between these two circumstances, and whether each of them considered separately bears any analogy to the strata which constitute the heights that border on these valleys.

The elucidation of each of these points they consider as necessary to render geology a body of doctrine, or to constitute it a real science; but they apprehend that nothing absolutely certain is known respecting any of them; every thing that has hitherto been advanced being of a loose and indefinite nature; each person having considered these subjects rather in their relation to some particular system, than according to impartial observation. Fossils alone, it is thought, will furnish matter for the study of several industrious philosophers for a space of 30 years; and their connexion with their different strata will supply a similar period with subjects of laborious research. The Report then

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concludes with some observations on the work which forms its subject; but for which we must refer such of our readers as would feel interested in the perusal of them, to the paper itself.

Observations.—Little need be said, we apprehend, to evince the importance of that branch of science (whenever it can justly claim the appellation), which treats of the internal structure of the earth, as least as far as human industry has been able to penetrate; of the nature and arrangement of the various materials of which it is composed, and of the changes that have taken place in these: thus, as it were, tracing the grand chain of connexion between the present and the primeval state of the globe we inhabit. The student of natural history cannot fail deriving a vast fund of both pleasure and advantage, from a science, which elucidates so extensive a department of nature. Whether the various minerals be considered in their individual, or in their collected state, which constitutes the strata of the earth, and composes the mountainous masses that rise above the general level of its surface, the variety of figure, and beauty of appearance, which many of them exhibit, as well as the uses to which they are applied, will always render them an interesting object of amusement and inquiry. The contemplative mind is delighted with that display of order and design which is manifested in the animal and vegetable kingdoms; while the mineral variety, the stupendous mountain, the towering cliff, the gloomy mine, the awful volcano, and the majestic cavern, present it with objects of the *beautiful*, the *grand*, the *terrible*, and the *sublime* And there needs no argument to shew that the proprietor of land, the miner, who searches for hidden treasures in the bowels of the earth, the philosopher, and the Christian, are all interested in this subject.

A very little acquaintance with the earth's surface is sufficient to convince us, that considerable changes have taken place in its physical constitution. The regular distribution of various strata, and, in many places, their extensive parallelism, appear to be the effects of general laws; while the irregularity of these strata in some places, and the total interruption of them in others, denote the influence of powerful and counteracting agents. The productions of the ocean exist on the summits of mountains, several thousand feet above the level of the sea, and the remains of organized bodies in the midst of the hardest rocks. This vast assemblage of apparent order and confusion, has given rise to a variety of attempts to trace out the causes by which these effects have been produced. The Germans took the lead in the modern improvements in geology, and Lehmann is the reputed father of this science. He was succeeded in his own country by Bergman, Ferber, Gmelin, Cronstedt, Born, and Werner. In Italy, the subject has been treated by Arduini and Tilas; in Switzerland, by Saussure and De Luc, and in Russia, by Pallas. France presents the names of Buffon, Delamétherie, Saint Fond, Dolomieu, and Lavoisier; and

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in Britain, Burnet, Woodward, Whiston, Whitehurst, James Hutton, Kirwan, and Playfair, are names familiar to the geological student. Most of the theories that have been advanced on the subject, manifest great ingenuity, and boldness of assertion, and do honour to the imaginations of their authors; but all that we can observe respecting them in this place is, that each has been applauded in its day, and generally refuted by its successor.

We fully agree with these learned reporters, that very little is yet known, with certainty, on the subject, notwithstanding all the advances that have been made within the last half century; and that the accumulation of facts is yet too small, and the facts themselves of too confined a nature, to serve as the basis of a rational system, and account for the revolutions which have operated upon our globe, and reduced it from its primitive to its present state. We, however, by no means consider the attainment of such a system impossible, nor doubt of its rising, in time to come, to a proportionate degree of perfection with the other acquirements of man. The inductive mode of considering the subject which MM. Haüy, Levierre, and Cuvier have pointed out, we conceive to be the only certain process which can conduct the geological philosopher to the full attainment of his desired end. Many of the objects to which they direct the attention of geologists, are of prime importance, and merit the closest investigation.

Professor Playfair, in his "*Illustrations of the Huttonian Theory,*" considers the proper object of a theory of this nature, to be that of tracing the series of revolutions which have taken place on the surface of the earth; to explain their causes, and thus to connect together, all the indications of change that are found in the mineral kingdom. He observes, that the formation of such a theory, requires an accurate and extensive examination of the phenomena of geology, and that it is inconsistent with any but a very advanced state of the physical sciences. There is, perhaps, no research in those sciences more arduous than this; none where the subject is so complex, where the appearances are so diversified, or so widely scattered; and where the causes that have operated, are so remote from ordinary observation. Dr. Kirwan, in his "*Geological Essays,*" has laid down some excellent laws of reasoning, which should be most strictly adhered to in investigations of this nature:—1st, That no effect should be attributed to a cause, whose known properties are inadequate to its production.—2d, That no cause should be adduced, the existence of which is not proved, either by actual experience, or approved testimony. Many natural phenomena have arisen or do arise, in times or places so distant, that well-conditioned testimony concerning them cannot, without manifest absurdity, be rejected: thus the inhabitants of the northern parts of Europe, who have never felt earthquakes, nor seen volcanoes, must nevertheless admit, from mere testimony, that the first have been, and that the second do actually exist.—

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3d, That no powers should be ascribed to any alleged cause, but those that it is known by actual observation to possess in appropriate circumstances. After all, however, which human acquirements will ever attain, the internal parts of the globe must remain unknown to us; and whatever relates to any thing beyond the mere surface, will be purely conjectural; as the greatest depths to which we can penetrate bear such an extremely minute proportion to the semi-diameter of the earth.

On the Limits of simple Vision and the Points of Correspondence of the Retina, &c. By Dr. Haldalt.—Journ. de Physique, July 1807.

Dr. Haldalt remarks, that the admirable sympathy which exists between the moving muscles of the eyes, and which constantly determines these organs to the same direction; the difficulty of fixing one of them without the other; and the almost absolute impossibility of moving them simultaneously in different directions, shew that they are destined to be affected in concert. Physiologists unanimously agree in acknowledging the necessity of a correspondence between the points of the retina that receive the simultaneous impression of the double image, in order to produce only one perception of the object. But in what does this correspondence consist? Is it established between points taken in the whole extent of the retina, between any, or between two only? These are questions respecting which, either authors have not agreed, or which have not been examined; and appear to merit some degree of inquiry. After stating the different opinions of Mariotte, Haller, and Lecat, on this subject, Dr. H. observes, that "between authorities so respectable, experiment ought only to decide. Those to which I have had recourse are the most simple: they consist, principally, in producing an artificial squinting, by changing the usual direction of the optic axes by mechanical means."

The phenomena which constitute the first results of these experiments, are such as cannot be reconciled with the opinion which restrains the position of this image to a single point at the bottom of each eye; for one of the impressions being necessarily made upon a point of the retina which is not destined to receive it, ought not to give rise to any perception. From this fact alone we may conclude that the place of the image is not necessarily at the vertex of the optic axis, but that many points on the retina, besides this, may receive the impression and afford perception. We may convince ourselves of this by shutting one eye, fixing the other with a speculum, and directing it to a candle placed in the prolongation of the optic axis, but in different angular positions. Its light ceases to be visible when the angle is 70°

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at least. We, however, only distinguish the candle perfectly, when the angle is much less; but we have no perception of the light when its position is such that none of the rays can attain the posterior hemisphere of the eye. It is therefore evident that nature has not given an extension to the retina in vain, and that the points situated about the optic axis, because they are less favourably posited, are not less susceptible of being affected than those in the axis. Light received into the eyes by inclining them so that the images make with the optic axis angles of 15°, 20°, or 25°, suffers no perceivable diminution in its brightness. These facts, which are easily proved, shew that the field of distinct and perfect vision is much more considerable than that assigned to it by Dr. Young in his learned memoir on this subject.

Dr. Haldalt conceives that it is not necessary to suppose that point of the retina which corresponds to the optic axis to be indued with greater sensibility than the other points, in order to render it the place of perfect vision; because it is the focus of refractive preparation, the only point where the image can have all its perfection; and that the most vivid impressions take place at this point, not because it is the only one capable of producing vision, but because it is the most advantageously situated, he considers as sufficiently proved both by artificial squinting, and by observation on the accidental squinting mentioned by Cheselden. As it appears that when the double image falls upon correspondent points of the retina, that is, upon those which possess similar functions, whatever be their situation, simple perception is produced, it becomes an object of inquiry, which points are capable of producing simple vision and which are not. This author observes, that the points of correspondence are generally situated at the vertex of the optic axis, have rigorously the same position upon the retina, or are symmetrical. Yet in the rapid motions of objects, in those of the eyes produced by the affections of the mind, in the accommodations of objects on this side of the point of distinct vision, in certain positions of the head; how is it to be conceived that the impressions are made upon parts that are exactly symmetrical? This simple reflection had long induced Dr. H. to suppose that the images might be received upon points of the retina which were not symmetrical, and yet not produce double vision: and experiment has confirmed this supposition. These experiments were the following:

If a lighted candle at the distance of eight or ten feet be viewed with one of the eyes; and after rendering the position of this eye invariable by means of a speculum, the other be pressed either on the inside or the out, so as to incline the optic axes to each other, a double image of the candle will be produced.

A double image may also be obtained by inclining the axes of the eyes to each other, either inwards or outwards, and by inclining them to the rays of the candle. The parallelism of the trans-

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verse axes of the eyes ought to be carefully preserved in those motions, in order to prevent the effects produced by them from being confounded with those arising from an inclination in the vertical plane.

If one eye be shut, the other directed to an object either above or below the candle, and then, after having fixed the open eye in that position, the other be suddenly opened, we shall have a double image of the candle. A slight inclination of the optic axes in the vertical plane gives rise to double vision; but it cannot be produced without a considerable inclination of these axes in the horizontal plane. A double image may be obtained in the horizontal plane, however, without employing any violence upon the eye, which affords reason for supposing any change to have taken place in the form of that organ. It is sufficient for this purpose to view an object placed at the height of the eyes, very obliquely; and the same may be obtained in an oblique plane by a certain inclination of the head; while it takes place in the vertical plane by mechanical means only.

Dr. H. then observes, that "these facts, from a great number of experiments repeated and varied in different ways, not only prove that nature has admitted a certain latitude in the law which she has imposed relative to the points of correspondence; that it is not absolutely necessary that the points of the retina upon which the impressions are made should always be similarly situated; but also that there are points of the retina taken at a distance from the optic axes, which may or may not produce double images, according to the directions in which they arrive at the eye. Thus, in the transverse plane, the optic axes ought to be much inclined, in order to give place to a double image, while a small inclination produces the phenomenon in the vertical plane. These general limits of the field of simple vision, of the area of the points of correspondence, present the elements of an ellipse, of which the greater axis is parallel to the transverse axis of the eye, the less axis parallel to its vertical axis, and the centre corresponds to the vertex of the optic axis.

"This general determination of the form of the fields of simple vision not appearing to me to be sufficient, I have made some attempts, in order to indicate more exactly its extent and its limits. The means which I employed consisted in comparing the angle formed by the double image, with the angle which it is necessary to form by the inclination of the optic axes in order to produce it. I have found that the apparent separation of the double image, in the horizontal plane, was at most but a third of what ought to result from the given inclination of the axes in the same plane; while in the vertical plane, the separation of the images and the inclination of the axes were sufficiently exact proportionals; from which I concluded that the extent of the field of simple vision was three times as great in the horizontal plane as it was in

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the vertical plane. But as I have found that the inclination of these axes in the former plane ought to be about 15°, in order to occasion a double image, the distance of the centre of the opening of the pupil, where the rays cross each other, to the bottom of the eye upon which the image is painted, being about twelve millimetres (about .47244 of an English inch) in an adult, I found that the opening of this angle ought to comprehend horizontally an extent of eight millimetres (.31428 in. Eng.) upon the retina: and a third of this dimension being assigned to the limit of double vision in the vertical plane, the field will be represented by an ellipse of which the axes are 8 and $2\frac{2}{3}$ millimetres respectively."

From these researches, this learned author is led to some reflections upon the perfection of the achromatic functions of the eye; and he examines the leading objections which have been urged against this theory. As the slight appearances of decomposition in the rays of light which the experiments present, are little sensible, and in some cases equivocal, he considers it as insufficient to invalidate the theory of this perfection; and among other arguments by which he supports his opinion are the following. Because this iris may be confounded with the penumbras that accompany all small bodies when seen indistinctly with the proper shadows, which in some circumstances are coloured; but chiefly because the feeble iris which is observed may be produced, not by the unequal refractive force of the humours of the eye upon different rays of light, but by the attraction of these bodies upon the luminous rays which approach their surfaces before they arrive at the eye; and this appears more evident, as in places of a black ground, which do not reflect the luminous rays, the iris absolutely disappears. In order to draw as a conclusion from this appearance the unequal refractive power of the eye, it is necessary to demonstrate that this decomposition cannot depend upon any other cause; this, however, he says, is evidently out of the eye, and independent of its action. In order too to conclude that the humours of the eye are not perfectly achromatic, the luminous rays should be applied to that organ as they come from the bodies which emit them, in such a manner as not to suffer any decomposition before they attain it; it should also be proved that this decomposition is effected in their passage through the humours of the eye. But this has not taken place in any of the experiments upon which the objections to this perfection are founded. Dr. H. therefore considers the perfection of the achromatic property of the eye as fully established and demonstrated.

Observations.—Of all our corporeal senses or faculties, we conceive that there is not one which affords us so many resources of utility and entertainment as vision; nor any from the loss of which we are subjected to so many, so various, and so aggravated cala-

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nities. By means of this power we are at once made acquainted with the forms by which bodies are limited, the colours by which they are embellished, the relative positions in which they are placed, and the motions by which they are conveyed from one situation to another. "It discriminates without confusion all those modifications that seem to sport in a thousand different ways in that grand diversity of objects to which a single look can extend itself." The nature of this faculty, and the mode of operation by which it is capable of conferring so many advantages, could not long elude the curiosity of man, or be regarded by him as objects unworthy of inquiry: and we conceive that a brief sketch of the leading opinions which have been entertained by different philosophers relative to these, will neither be uninteresting nor uninstructive to many of our readers.

The Platonists and Stoics supposed that external objects were perceived by means of rays passing from the eyes to the object, by which they were modified and returned to the eyes, where they produced the sensation. The Epicureans maintained that vision was effected by the emanation of corporeal images from the objects to the eyes; or by a kind of atomical effluvia constantly emanating from whatever could be perceived by this organ. The Peripatetics differ from the Epicureans in conceiving the images or species to be incorporeal. With respect to the mode by which the sensation is effected, they assert that every visible object impresses its image on the contiguous air; and this image another, somewhat less, on the next film of air; and so on in succession, until the last is impressed upon the crystalline humour of the eye, which they regarded as being the immediate mover of the soul. These species they denominated *intentionales* or *intentional species*. John Baptista Porta of Naples, a celebrated philosopher and mathematician for the time in which he flourished, was the first who proved, about the middle of the 16th century, that vision was not caused by rays emitted from the eyes, but by means of light received into the eye. The Cartesians and Newtonian philosophers give a better account of vision. They say it is performed by rays of light, reflected from the several points of visible objects, received into the pupil, and refracted and collected in their passage through the humours to the retina, where the impression or image is formed, and whence it is conveyed to the brain by the capillament of the optic nerve.

We know from experience that the eye is capable of viewing objects at a certain distance without any mental exertion; and that this distance varies with different eyes. Beyond this distance no mental exertion can be of any avail; but within it, the eye possesses the power of adapting itself to the various occasions which may occur, and to which it may be directed by the volition of the mind. In what manner this is effected, is an inquiry which has frequently engaged the attention of optical philosophers. Dr.

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Young has examined the chief solutions of this problem presented by different writers, and offered some observations of his own, in an ingenious paper printed in the Philosophical Transactions for 1793, and reprinted in the second volume of his Natural Philosophy; and from which several of the succeeding remarks have been obtained.

Kepler was the first who gave a theory in order to account for this accommodation of the eye. In this theory, he supposes the ciliary processes to contract the diameter of the eye and lengthen its axis, by a muscular power. Descartes imagined the same contraction and elongation to be effected by a muscularity of the crystalline, of which he supposed the ciliary processes to be tendons. Dr. Young, however, asserts that neither of these suppositions is adequate to the effect. De la Hire maintains that the eye undergoes no change except the contraction and dilatation of the pupil. This position he rests on an experiment which has been shewn by Drs. Porterfield and Smith to be fallacious. Haller, too, adopted the same opinion, notwithstanding its inconsistency with both the known principles of optics, and constant experience. Dr. Pemberton supposed the crystalline to contain muscular fibres, by which one of its surfaces was flattened, while the other became more convex. But Dr. Jurin has proved that a change of this kind could not produce the effect. "Dr. Porterfield conceives, that the ciliary processes draw forward the crystalline, and make the cornea more convex. The ciliary processes are, from their structure, attachment, and direction, utterly incapable of this action; and by Dr. Jurin's calculations, there is not room for a sufficient motion of this kind, without a very sensible increase in the length of the eye's axis: such an increase we cannot observe." The hypothesis which Dr. Jurin offers on this subject is, that the uvea, at its attachment to the cornea, is muscular, and that the convexity of the cornea is occasioned by the contraction of this muscular ring. Dr. Young observes that "no such ring is discoverable," and adds, that the same author's "theory of accommodation to distant objects is ingenious, but no such accommodation takes place." Musschenbroek conjectures, that the relaxation of the capsule of the vitreous humour, where it receives the impression of the ciliary processes, permits the coats of the eye to push forwards the crystalline and cornea. Such a voluntary relaxation, Dr. Y. observes, is wholly without example in the animal economy. Some have supposed that the pressure of the external muscles lengthens the axis of the eye: but this is insufficient for the purpose intended. Others, but with less reason on their side, have said that the muscles shorten the axis. Nor have those been wanting who have advanced that the ciliary processes flatten the crystalline, inconsistent as it is with observation. "Some other suppositions have also been formed by different physiologists. Zinn imagined the ciliary processes to be distended by a fluid, and

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to protrude the lens. Sauvages conjectures, that the ring of the petit is inflated by the electric fluid, and alters the form of the lens: Moulin, that the cornea is rendered more convex by its ligaments, which are in fact nerves: Bourdelot, that the contraction of the pupil increases the convexity of the lens. But all these suppositions are liable to as strong objections as those which I have already examined."

Dr. Young then proceeds to investigate a solution of his own. He imagined that no other power was capable of producing this change but a muscularity of a part, or of the whole of the capsule of the crystalline lens. But on closely examining the eye of an ox both with the naked eye and a magnifier, he conceived that he had discovered an arrangement of fibres which seemed to remove the difficulties with which this branch of optics had long been embarrassed. This muscular mass, for such he considers it, he observes "is inclosed in a strong membranous capsule, to which it is loosely connected by minute vessels and nerves; and the connexion is more observable near its greatest circumference. Between the mass and its capsule is found a considerable quantity of an aqueous fluid, the liquid of the crystalline." Dr. Young then enters into several particulars to prove the action and utility of this structure. Dr. Hosack, however, in the Philosophical Transactions for 1794, contests the very existence of these muscular fibres. Amongst other reasons urged by him are, the great degree of transparency they must possess, not to occasion an irregularity in the refraction of those rays which pass through their several parts; and their number, which must amount to twelve thousand. After urging other objections to Dr. Young's hypothesis, he concludes that no such muscles exist, and that Dr. Y. must have been deceived by some other appearance which resembled muscles. Dr. Hosack then states it as his opinion that this accommodation of the eye is effected by means of its external muscles. He thinks that it is neither inconsistent with the general laws of nature, nor contrary to the animal economy, to suppose that these muscles, from their combination, should have an additional action and utility, exclusive of the common purposes to which they are well known to be subservient. In this paper Dr. Hosack inquires into the structure and use of these muscles, and the manner in which they are capable of producing the requisite changes; and illustrates his hypothesis by several experiments.

The subject was afterwards pursued by Mr. Home, with the assistance of Mr. Ramsden and Sir H. Englefield. Mr. Home endeavoured to prove that the accommodation was principally effected by an increase of the convexity of the cornea, which he imagined he had actually observed. But on repeating his experiments in a different manner, he was obliged to abandon this opinion in some measure, and to suppose that only about one third of the change was produced by an alteration of the cornea,

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and the remainder he imagined was to be attributed to an elongation of the eye.

In the paper quoted by Dr. Haldalt, which was read to the Royal Society in 1800, Dr. Young has shewn, by examining the changes of the eye when the cornea is in contact with water, that the figure of the cornea is not in any degree concerned in the accommodation: he has also made a number of different experiments in order to prove that the length of the eye is wholly incapable of variation; and others to shew in a direct manner that the form of the crystalline is actually changed, and its curvature often rendered irregular. In this paper he mentions collaterally the extent of perfect vision, observing that "the visual axis being fixed in any direction," he "can at the same time see a luminous object placed laterally at a considerable distance from it; but in various directions the angle is very different. Upwards it extends to 50°, inwards to 60°, downwards to 70°, and outwards to 90°: these internal limits of the field of view nearly corresponding with the external limits formed by the different parts of the face. It is well known that the retina advances further forwards towards the internal angle of the eye, than towards the external angle; but upwards and downwards its extent is nearly equal, and is, indeed, every way greater than the limits of the field of view, even if allowance is made for the refraction of the cornea only. The sensible portion seems to coincide more nearly with the painted chord of quadrupeds: but the whole extent of perfect vision is little more than 10°. The imperfection is partly owing to the unavoidable aberration of oblique rays, but principally to the insensibility of the retina: for, if the image of the sun itself be received on a part of the retina remote from the axis, the impression will not be sufficiently strong to form a permanent spectrum, although an object of very moderate brightness will produce this effect when directly viewed." In fact, it is easy to observe, that if we fix our eye on any word of a book printed in a small type, we cannot read while it remains in the same position, the words which are an inch or two above or below it, or on either side, although we form a very good general idea of the figure and brightness of objects much more remote from the axis. Dr. Haldalt could scarcely have objected to this statement if he had considered it in its entire state. Dr. Young has also shewn that the imperfection of the focus throughout the area of the retina, is probably in most eyes considerably less than occurs in his own eye in the optical axis, on account of an obliquity of the crystalline, which, however, does not prevent his having very perfect vision of minute objects.

Dr. Haldalt's arguments and experiments respecting double and single vision, seem only calculated to create obscurity in a subject which in itself is very simple and clear. Each eye has a point of the retina not symmetrically, but similarly situated in the other eye: and the correspondence between every pair of points is such,

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that when similar sensations affect both points at once, they are confounded, and appear to the perception but as one. We can discover nothing in Dr. Haldalt's experiments that tends to invalidate the existence of a rigorous correspondence of this kind, or to shew that it is susceptible of any modification.

It is extremely singular that Dr. Haldalt should hold up a mere opinion or conjecture, that the various substances composing the eye are so arranged as to render it achromatic; not even founded on any reasoning respecting the necessity or utility of such an arrangement, in opposition to the evidence of positive and repeated experiments. Dr. Wollaston has measured the dispersive powers of the crystalline lens, and of the other substances composing the eye, and has found them even greater than those of many other natural substances; the same philosopher has observed a striking difference in the actual refraction of rays of different kinds; and Dr. Young has even measured that difference in various ways; and yet Dr. Haldalt insists that the actual determination of this dispersion is "insufficient to invalidate the theory" of the imaginary perfection of the eye; notwithstanding that Dr. Maskelyne had long before demonstrated, that the imperfection produced by dispersion, must necessarily be too small to occasion any practical inconvenience. On such a disposition to prefer speculations to experiments, it is unnecessary to make any farther comments.

Researches concerning Heat produced by Friction. By Dr. HALDALT.—*Journ. de Physique, Sept.* 1807.

AFTER offering some reflections on the nature of heat or caloric, and briefly noticing the leading opinions which have been held by different philosophers respecting it, Dr. Haldalt informs us that he undertook the subsequent experiments with a view of adding some facts to those which might one day serve perhaps more completely to illustrate this important question. The experiments of Count Rumford, though made on a large scale, did not appear to him to have been sufficiently varied, and his object was, therefore, not only to make use of bodies of different natures, but also to modify all the circumstances which might concur in developing heat during the progress of his experiments.

The apparatus with which these experiments were made, consisted of a cubical box of oak, joined together very firmly and cemented, in which a vertical axis revolved, having its lower extremity resting in a socket of copper fixed to the bottom of the box; the middle part carried a pulley firmly attached to the axis: the upper extremity of this axis also turned in a kind of copper socket fixed to the lid of the box; finally, towards the lower part of the axis, a mass of copper provided with angles was fixed, for the purpose of retaining the cylindrical pieces of metal which were

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adapted to them. The diameter of each of these pieces was about 6 cent. 5 millim. (about 2.55906 inches Eng.) The friction was produced on the convex surface of these hollow cylinders by a spring supported horizontally in the interior of the box. The spring received at one of its extremities rubbers of metal adjusted to a groove; at the other end a pressing screw, which crossing the box, imparted to it a sufficient degree of tension to cause it to press against the surface of the cylinder. A graduated arc adapted to the spring, indicated in weight the force produced by its tension. The motion which produced the continued friction was communicated by an endless cord or belt, which passed over the pulley fixed on the vertical axis, and connected it with another revolving wheel or large pulley. The diameters of these pullies were to each other as 1 : 4, so that if the greater was caused to make one revolution per second, the smaller would make four; and as the axis which supported it revolved with the same rapidity, a friction was produced, of which the velocity was more than 84 centimetres (32.971 in. Eng.) in the same time. This velocity was sufficient to produce sensible effects, and was that employed by Dr. H. in his experiments. The other parts of the apparatus were hollow cylinders of equal surfaces, but of different natures, and rubbers (*frottoires*) of copper and steel 3 centim. in height.

The heat produced by the friction of this apparatus was employed in raising the temperature of a quantity of water of 3 decig. 664 cent. (5.66 grains troy), which was contained in the box, and its temperature was measured by a thermometer immersed in it. The water employed in these experiments had generally a temperature but little different from that of the air where they were made at the commencement of the operation; and in order to diminish the influence of the air upon it during that time, that which was in contact with the water was prevented from being renewed, and the time of the operation abridged.

Experiments.—1. The first experiment was made with a cylinder and a rubber of brass. The cylinder and rubber being weighed exactly, both in air and water, before and after the operation, the spring pressed with a force equal to a weight of 20 kilog. (53.625 lbs. troy), and the large pulley caused to make 60 revolutions per minute, the temperature of the water was found to be raised 2° of Réaumur's thermometer for each 15 minutes; so that after a duration of 70′, a surface of 3 centim. in breadth and 21 cent. long, by rubbing with a velocity of 84 cent. per second, had produced a quantity of heat capable of raising the temperature of the water 9°, although the quantity of metal detached from the surface was less than half a grain, and the condensation unappreciable.

2. In this experiment a cylinder of lead was substituted for that of copper, and the same precautions taken as before. The temperature of the air at the commencement of the operation was 9°,

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and that of the water 7°; and this last at the termination of each succeeding period of 15 minutes, as observed by the thermometer, was 10, 12, 14, and 16 degrees respectively. When the operation had been continued for 75′, neither the cylinder nor the rubber had suffered any diminution in either weight or bulk. From this experiment it appears, that, without undergoing any change, a cylinder of lead, of which the density is to that of copper as 11.352 to 7.788, had produced an equal quantity of heat.

3. The third experiment was performed with a cylinder of pewter, the density of which to that of lead was as 7.291 to 11.352. All the circumstances were the same, except that the temperatures of the air and water were each 11°; and at the expiration of each 15′ the temperature of water offered the following progression, 13°, 15°, 17°, 18°; from which it appears that the cylinder of pewter had yielded $\frac{1}{2}$ of the heat produced by that of copper, while this last had given an equal quantity with that of lead, although its specific gravity was only about $\frac{7}{11}$ of this latter metal.

4. A cylinder of zinc was then substituted for that of pewter, and the water taken at 10°, all other circumstances being as before, the following progression was obtained, 12°, 14°, 16°, 18°, 20°. This cylinder, therefore, although less dense than either that of lead or copper, had yielded a greater quantity of heat.

5. In this experiment a cylinder and rubber of brass were employed, and the pressure reduced to 10 kilog. The temperature of the water was raised only 1° in 30 minutes.

6. The pressure being increased to a force equal to 40 kilog. the temperature acquired by the water in the same time, as in the last experiment, was found to be six times as much, or about 7 degrees.

7. The cause to which the developement of heat occasioned by friction is most commonly attributed is the abrasion of the surfaces in contact, from which the molecules are detached with violence. With a design of proving this influence, Dr. H. employed a rubber of steel, cut in the manner of a file, and applied to the surface of a cylinder of copper with a force of 20 kilog.: after having continued the operation for the space of 60′, an equal quantity of water was raised from 14° to 18°; and the cylinder had lost 3 decagrames (463.3 grains) of its weight. This quantity of metal is a thousand times greater than that lost by the same cylinder against a rubber of polished copper, in the 1st experiment, and it only afforded half as much heat in the same time, notwithstanding the pressure was the same.

8. This experiment was instituted for the purpose of ascertaining the influence that would arise from having the apparatus surrounded by bodies which were good or bad conductors of heat.

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It was therefore placed in a box of deal, and supported at the distance of a decim. (3.93702 in. Eng.) from all its sides, by pieces of half-burned wood: the interval between the boxes was filled with small charcoal. The cylinder and rubber were of brass, and the pressure and velocity as in the last experiment. The time was divided into three equal portions of 30′ each. In each of the periods the temperature acquired by the water in the inner box was pretty regular, and about 3°; although the communication was alternately interrupted and established. By comparing this experiment with the first, it appears that the quantity of heat produced in the same time was less by one third than it was in that experiment.

9. As the electric fluid, as well as caloric, is produced by friction, and is generally propagated with more facility, and as these two fluids possess some other analogies, it was supposed that the former had some influence upon the production of the latter, and the preceding experiment was repeated in order to appreciate this hypothesis. The apparatus was insulated by placing it in a large deal box, well dried and covered with resin, and in which it was supported at the distance of a decimetre from each of its sides, by pieces of wood, that had been dried in an oven and dipped while hot in a cement of boiling gum-lac: the whole was supported on an insulated stand. The experiment was continued for an hour, which was divided into four equal parts, and the communication with the common reservoir was alternately established and interrupted; and each period offered an equal quantity of heat. During the 60 minutes the water acquired only 6° of temperature; from which it appears that from being insulated either by idio-electric substances or imperfect conductors of heat, the quantity of heat produced by friction is diminished.

10. The cause which appears to have exercised the greatest influence on the production of heat in these experiments is the condensation of the particles of the different bodies submitted to the operation of friction by the pressure employed; and to this cause also it was ascribed by M. Berthollet. In order to determine this point, Dr. H. caused a strong oak box to be made and covered with a cement impervious to water, and capable of containing just a cubic decim. of this liquid. On the bottom, which consisted of the end of a solid piece of wood, was fixed a piece of steel, and two of the opposite sides of the box were cased with leather, so as to permit an iron wire to pass through the box without suffering the water which it contained to run out. A part of this wire, which was about one third of a line in diameter, rested upon the small steel anvil; and was subjected to compression by means of a hammer which passed through the lid of the box, rested upon the wire, and was driven by a hammer. Each stroke reduced

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that part of the wire upon which it was exerted to a flat state, and to a little more than a line in breadth; its length was increased, but its specific gravity remained unchanged. The temperature of the water employed to collect the caloric disengaged by the condensation was raised only 2°.

If these experiments have not been so successful as might have been wished, in determining the cause to which the production of heat by friction ought to be attributed, they have not been altogether destitute of utility: they have proved that the quantity of heat thus produced is very considerable; that it is modified by the nature of the bodies which are submitted to the operation; and that it is neither in the ratio of their surfaces, nor in that of the number of molecules detached from them.

The influence of pressure has been proved by the 5th and 6th experiments, and as its effect upon compressible bodies is necessarily to condense them, does it not seem to indicate the approximation of their molecules as a cause of the disengagement of heat? But as the production of heat ought to be greater as the same pressure is exercised upon a smaller number of points at the same time, it would appear that the 7th experiment, in which a considerable number of molecules were detached, ought to have produced a correspondent quantity of heat, whereas the quantity was only about half of what was produced by the friction of polished surfaces. The influence of condensation is rendered yet more uncertain by the 10th experiment, in which the compressive force was still more powerful; the condensation was greater than that effected by the simple pressure, and the quantity of heat disengaged was less than in the other experiments.

By admitting that the calorific phenomena, produced by friction, depend upon the caloric expelled from the pores by the approximation of the particles of the compressed body, how is it that the molecules, in re-establishing themselves in their former state, as in the case of elastic metals, such as zinc, do not re-assume the quantity of caloric which had been set free by their compression? And if this hypothesis explain the small degree of heat obtained by the 10th experiment, how does it apply to the greater quantity produced by the 1st, 2d, and 3d experiments? From discussing these facts, Dr. H. observes, that "we are led to the following consequences: whether the calorific phenomena produced by friction depend upon a particular fluid put in motion by that action, or whether this fluid be expelled from the pores of the metal by condensation, or be drawn from the surrounding bodies, like the electric fluid. In the first case, the heat ought to diminish by condensation, to follow the inverse ratio of the density, and to be exhausted; in the second, it ought to be modified by insulation of the bodies submitted to the operation of friction: this neither took place in these experiments, nor in

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those of Count Rumford. If, on the contrary, these phenomena are produced solely by the intestine agitation of the molecules, the quantity of heat ought to diminish by condensation, to present some proportion to the density, and, above all, to the elasticity of the metal. Such are the difficulties which still attend the question concerning the cause of heat produced by friction, and which requires new experiments. It is sufficient for me to have approached them, and pointed out their importance."

Observations.—We fully agree with this writer respecting the difficulty of ascertaining the exact manner in which the heat arising from friction is produced; as well as in his opinion that all the experiments hitherto instituted for the purpose are inadequate to the investigation. Mr. Dalton, who has adopted an opinion nearly similar to that of M. Berthollet, states, in the first part of his "*New System of Chemical Philosophy*," that "the heat produced by the percussion and friction of solid bodies, arises from one and the same cause, namely, from a condensation of volume, and consequent diminution of capacity of the excited body; exactly in the same manner as the condensation of air produces heat. It is a well-known fact, that iron and other metals, by being hammered, become hot and condensed in volume at the same time; and if a diminution of capacity has not been observed, it is because it is small, and has not been investigated with sufficient accuracy. That a change of capacity actually takes place cannot be doubted, when it is considered that a piece of iron once hammered in this way, is unfitted for a repetition of the effect, till it has been heated in a fire and cooled gradually." In what manner this opinion of Mr. Dalton and Dr. Haldalt's experiments and inferences are to be reconciled, future experiments may perhaps determine; to us they appear at present to stand in direct contradiction to each other: nor do we see the necessity of supposing, with Dr. Haldalt, that if heat consisted in the motion of a particular fluid, it should either be diminished by condensation, or depend in any degree on the aggregate elasticity of the heated body.

An Essay on the Cause which affects the Surface of Fluids contained in capillary Tubes. By C.-J. Lehot.—*Journ. de Phys. Sept.* 1808.

The convex or concave surface of a fluid contained in a capillary tube has hitherto been attributed to the difference of the adhesion of the fluid molecules for the matter of the tube, compared with that of the same molecules for each other. M. Lehot extracts passages from some of the most modern and cele-

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brated works on the subject, in confirmation of this assertion, viz. from those of *Hauksbée*, *Baruel*, *Haüy*, and *Laplace*; and then observes that this opinion is generally adopted, and passes at present for an incontestable principle. Yet however strong the authorities which are here cited may be, and however satisfactory the consequences which are drawn from this principle, as above announced, he thinks that the facts contained in this Essay demonstrate that it is to another cause which this hydrostatic phenomenon ought to be attributed, instead of that to which they have been ascribed by the eminent philosophers above mentioned.

Experiments.—1. If after having placed a plate of glass horizontally at the bottom of a vessel, we place upon it a small globule of mercury, it will retain its globous form, and not spread upon the glass. Precisely the same result will be obtained, if, instead of the air which the vessel naturally contains, there be substituted either oil of turpentine or oil of olives.

2. If the vessel be partly filled with mercury, and a plate of glass applied upon the surface of the fluid, then, if a bubble of air be suffered to escape from the bottom of the vessel, it will rest at the inferior surface of the glass, and extend, instead of preserving its spherical form.

3. If water, oil of turpentine, or oil of olives, be put indifferently into a vessel, and a plate of glass applied to the surface of the fluid, a bubble of air escaping from the bottom of the vessel will ascend to the lower surface, where it preserves its globular form.

4. If a drop of olive oil be let fall upon mercury, it expands; but it spreads still further when a plate of glass is applied to the surface of the mercury.

5. If a small quantity of olive oil be placed upon a plate of glass in the air, it runs upon it.

6. When a drop of olive oil is thrown upon the surface of tranquil water, it expands promptly, and is reduced into a very thin film; if a plate of glass be then applied to the surface of the liquid, the oil is instantly collected and formed into a globular mass.

7. A drop of oil of turpentine put upon the surface of tranquil mercury is diffused by the application of the glass plate.

8. A drop of oil of turpentine placed upon a horizontal plate of glass, surrounded with air, extends upon the glass.

9. When a drop of oil of turpentine is thrown upon water, it extends with astonishing rapidity, and covers almost the whole surface of the fluid; but if the glass plate be applied to the surface, the oil is instantly collected and formed into a new drop.

10. If a drop of water be put upon the surface of mercury, it spreads, and the application of the plate of glass to the surface of the metal causes it to spread still more.

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11. If we throw a drop of water into either oil of turpentine or olives, it is a long time in descending to the bottom; but it ends by attaining it; and if the bottom be formed of a horizontal plate of glass, it loses its globular form and spreads above it.

12. A drop of water surrounded with air, and placed upon a horizontal plate of glass, spreads upon the glass.

There results from these facts, 1st, that the following fluids, water, oil of turpentine, oil of olives, air, mercury, are here so arranged that a small portion of any one of them placed upon a horizontal plate of glass, and surrounded with one of those which precede it in this list, does not spread upon the glass, but takes a form more or less approaching to spherical.—2. If two of these fluids be in contact by horizontal surfaces, and a capillary tube of glass be immersed in them, so that one of its extremities may be entirely within one of the fluids and its other extremity within the other fluid, that which is first in the above order will take a concave, and the other a convex form. This is confirmed by the three following experiments.

13. If a capillary tube be immersed partly in water and partly in oil of turpentine, the water will take a concave form, and the oil of turpentine a convex form at its surface: oil of olives substituted for that of turpentine gives the same result.

14. If a tube be immersed partly in oil of turpentine or of olives, the rest remaining in air, the oils take a concave, and the air a convex form.

15. If a capillary tube of glass be put partly in oil of olives and partly in mercury, the latter assumes a convex, and the former a concave form.

If there were only one fluid, affected solely by gravity, its surface would remain a plane, but this form is disturbed by the force which the tube exercises perpendicularly to its axis upon the parts of the fluid near its internal surface. In effect, by the property of fluids to press equally in all directions, this force is transformed into another parallel to the axis, which proves a resistance to part of the molecules adherent to the inferior circle of the tube, whence it results that the columns near the surface of the glass ought to be longer than the central columns; that is, the surface of the fluid ought to be concave. This experience confirms, since the mercury in a barometer perfectly purged of air and humidity, is terminated by a surface a little concave.

If we suppose, as in the preceding experiments, that there are two fluids in contact which are affected only by the same gravity, then they will be separated by a horizontal plane; but if we suppose that the tube exercises upon them a force perpendicular to its surface, which may be transformed, as above, into two opposite forces, then the fluid the most attracted by the

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difference of these two forces will have its columns near the glass lengthened, and assume a concave surface. There may therefore be added to the other principles developed in this Essay, that the fluids, water, oil of turpentine, oil of olives, air, mercury, are so disposed that each has a greater affinity for glass than those which follow, and less than those which precede it.

It also appears, that the convexity or concavity of the surfaces of fluids in the vicinity of solids, partly immersed in them, does not serve as a distinct character, as most modern authors have supposed, of the fluids, which either wet, or do not wet, any body whatever; this distinction is not exact, since all solid bodies are susceptible of being wet by any fluid whatever.

M. Lehot then adds, first, That the explanation which he has given relative to the convex or concave form of the surfaces of fluids in capillary tubes, by referring it to *the difference of affinity of the fluid for the matter of the tube, compared with that of the ambient fluid for the same tube*, is new, and appears to deserve the attention of philosophers. Second, That the classification of different fluids which he has established, relatively to their affinity for glass, affords a wide field for experiments, which may reflect an important light upon the register of chemical statistics and dynamics.

Observations.—It does not seem to have occurred to M. Lehot that the surface of the mercury, in the tube of a common barometer, is not in contact with air, but with a vacuum, if the expression may be allowed; and that, according to this mode of reasoning, a vacuum must be considered as having a greater affinity for glass than mercury; or, in other words, that mercury must actually be repelled by glass, which is contrary to the most obvious fact. M. Lehot has advanced no experiment that is inconsistent with the theory which has been lately established by Dr. Young, and by M. Laplace; he ought to have shewn the insufficiency of the explanations which have already been given, before he attempted to substitute a new one, even if it had been equally applicable to the phenomena. We are at a loss to understand the assertion, that all solids may be wetted by any kind of fluid: if the author means that a piece of solid steel can be wetted by a portion of clear mercury, we can only suppose that he has never tried the experiment. With respect to the action which takes place at the common surface of two fluids, it seems to be impossible to determine, from reasoning only, whether its magnitude is to be considered as proportional to the difference of the squares, or the square of the difference of their densities, or from the joint ratio of the difference and the greater density, which is a supposition intermediate between the two former. The best way of investigating this subject by experi-

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ment would be, to ascertain the angles of contact, formed by glass with mercury, in the presence of other fluids of different capillary powers: but there will probably long be some difficulty in reconciling the results of such experiments with any accurate laws of attractive forces.

An Abstract of a meteorological Journal for the Years 1807 *and* 1808, *kept at Middleshaw, near Kendal, in Latitude* 54° 20′. *By* JOHN GOUGH, *Esq.—Phil. Journ. No.* 99.

Mr. Gough informs us, that his object in the present communication is to call the attention of meteorologists to two points in this science which appear to have been too superficially attended to. These are, the diurnal variation of temperature at different times of the year, and the comparison of the rain collected at different elevations in the same neighbourhood. With respect to the first of these, Mr. G. observes, that it evidently arises from the influence of the sun in his passage from tropic to tropic; but this influence is affected by the vicissitudes of the weather, which renders this variation irregular; as it is greatest when the atmosphere is clear, and least in the contrary state. If the earth were destitute of an atmosphere, two maxima and two minima of variation would take place at stated times in each year; the former would happen about the equinoxes, near the time of mean temperature, and the latter would coincide with the hottest and coldest days. These extremes, as well as the time of greatest heat and cold, with that of mean temperature, do not appear, from the table he has subjoined, to be periodical.

In order to attain the second point referred to above, Mr. Gough made use of two gauges of the same construction and dimensions: one of them he placed in a valley, the other upon the top of a hill north of the former, and about 91 yards above it; the direct distance between them did not exceed 500 yards. From the annexed table it appears that the quantities of rain obtained by the two gauges differed less in summer than in winter: the whole results for the latter year, which Mr. Gough deems the most accurate, were, in the lower gauge, 38.631 inches, and in the upper 29.142 inches. The greatest quantity of rain fell each year in October, and the least in March; the former of these is said to have been 6.382 inches in the lower, and 5.19 in the upper gauge for each year; the latter was 1.798 and 0.122 for 1807, and 0.160 and 0.122 in for 1808. The greatest temperature happened each year in July; in the former the thermometer attained 69.14, in the latter 71.52; while the mean temperatures of these months were 62.12 and 63.56. The least height of the thermometer in 1807 was in March, and was

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30.29; that of 1808 happened in February, and was 31.16. The mean annual temperature is stated at 45.642 for the former, and 46.73 for the latter year.

Observations.—The two points which Mr. Gough wishes to press upon the attention of meteorologists are of considerable importance, and deserve to be attentively examined by every cultivator of that science. It appears rather singular that the quantity of rain obtained in the lower gauge for each of the last five months in both years should exactly correspond, as well as that for every month in the upper gauge, and the total in both gauges for each year. This coincidence of quantity we can only account for by supposing an error, either in the transcribing or the printing; of this, however, no notice has been taken in the succeeding numbers of the Journal. The greatest temperature at London, in July 1807, was 82 degrees; that for 1808 at the same place was 92°. Between these heights and those observed by Mr. Gough at Middleshaw, near Kendal, the differences are 12.86° and 20 48°. The lowest point to which the thermometer fell at London, in March 1807, was 18 degrees; on the 12th of January it was as low as 13°: in February 1806 the least temperature was 12 degrees. From this it appears that the difference of temperature between London and Middleshaw, which is upwards of three degrees north of it, is very remarkable. The former place seems to be much warmer in summer and colder in winter than the latter; and we are wholly at a loss to account for this last difference, unless Mr. Gough's thermometer was fixed in a favourable situation, and he observed its height at noon: this, however, he has not stated, but has denominated it the least temperature, which could not be at that time.

In the present state of the meteorological science it is certainly desirable that registers should be kept at a great variety of places; and the various circumstances and changes carefully noted as they take place. The body of facts thus collected during a considerable period, would probably furnish matter, if well digested, for a more rational theory than exists at present on the subject. Mr. Gough would have added to the value of his information relative to the rain-gauges if he had stated their heights above the level of the sea; as this might have afforded a means of comparison between these and others, of which the heights above the same level were known. The whole quantity of rain that fell at London in 1807 was 14.12 inches, which is less than half of that which Mr. G. obtained in his upper gauge. The greatest quantity fell in May, and the least in December; the former of these was 2.88 in. and the latter 0.24.

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On ascertaining square Numbers and Biquadrates by Inspection.
By W. Saint, Esq.—Phil. Journ. No. 99.

After remarking that the circumstance of not being able to distinguish readily which are square numbers and which are not, frequently occasions considerable trouble in the resolution of equations; Mr. Saint, with a view of obviating at least a part of this inconvenience, gives the following propositions, corollaries, and scholium for ascertaining what numbers are *not* squares.

Propositions.—"1. A square number cannot terminate with 2, 3, 7, or 8.

"2. A square number cannot terminate with an odd number of ciphers.

"3. If a square number terminate with 4, the last figure but one will be an even number.

"4. If a square number terminate with 5, it will terminate with 25.

"5. If a square number terminate in an odd number, the last figure but one will be an even number: but if it terminate in an even number, except 4, the last figure but one will be an odd number.

"*Corollary.*—Hence no square number can terminate with two equal figures, except two fours or two ciphers.

"*Prop.* 6. A square number cannot terminate with more than three fours.

"*Cor.* Hence no square number can be contained under any number of equal digits.

"*Scholium.* If this speculation be extended to cube numbers, it will be found that such numbers may terminate with any of the nine digits; and that it is not therefore so easy to ascertain whether a number be a perfect cube from the nature of its terminating figure.

"With regard to biquadrates, or fourth powers, we may observe, that, as these are square numbers also, whatever has been demonstrated above, relating to square numbers, excepting Prop. 1, holds equally true of biquadrates. We may further remark, that biquadrates terminate in 0, 1, 5, or 6; and that, when they terminate in 0 or 5, their roots will terminate with 0 or 5 also; moreover, that when the root terminates in 5, the biquadrate will terminate in 625."

Observations.—To such of our readers as are much employed in the extraction of the square root, these propositions may sometimes, if well fixed in the memory, be found useful, by rendering the operation unnecessary, particularly when a *rational* root is the object to be attained; and their demonstrations will be easily supplied by the attentive student.

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1. *Plan for an improved Theatre. By Sir* GEORGE CAYLEY, *Bart. —Phil. Journ. No. 99.*
2. *On the Construction of Theatres. By* RICHARD LOVELL EDGEWORTH, *Esq. F.R.S. and M.R.I.A.—Phil. Journ. No. 102.*

1. IN the construction of a theatre, the architect's chief aim should be the gratification of both the sight and hearing of the greatest number of persons as completely as possible. Hence, economy of space, regulated by the science of acoustics, demands his closest attention. These are the author's leading principles in this improved plan, and in his illustration of the manner of carrying them into effect, he has made choice of an elliptical form; because that is not only a beautiful curve, but possesses the property of reflecting the sound uttered in one of its foci to the other, and the distant parts of the theatre only require this aid. The stage occupies one focus, and is drawn semicircular; and it is conceived that material benefit would arise to the audience from having the box seats concentrical with it, but it would be disadvantageous in point of accommodating numbers. The scenes, "*if conveniently practicable*," should be segments of circles; and if double their height can be had within the building, they might be suspended by cords and pullies. A line drawn from the top of the scenery to the eye of the most backward person in one tier of boxes, should just clear the bottom of the next higher tier.

The circumstance of the theatres in the metropolis being larger than the direct voice of an actor can fill with ease, renders it necessary to call in the assistance of reflected sound; and therefore to construct the building so that this shall be obtained in the greatest degree in those places where it is most wanted. With respect to the nature of this reflection, Sir George Cayley observes, "no similar sounds uttered in succession to each other, can be heard by the finest ear, when the interval of time between each does no exceed $\frac{1}{10}$ of a second; therefore, no reflected portion of a sound that arrives at the ear within less than $\frac{1}{10}$ of a second after the direct impulse will make a separate impression or echo, but will add to the strength of the former impression. Hence, as sound travels about 1142 feet per second, unless the difference of the whole distance travelled over by the reflected impulse and that of the direct impulse be greater than 114 feet, no echo will be perceived. In the plan of the theatre sent, the greatest difference of this sort only amounts to 68 feet; therefore, even in this instance, about one half of the influence of the reflected pulse would be exerted in adding to the intensity of the sensation of the direct pulse, and about one half in prolonging a nearly similar

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impression." In this plan, the distance from the centre of the stage to the extreme part of the pit is 120 feet; and the direct voice is supposed to be sufficient to fill within one fourth of the extremity of the building; and the roof is so curved, as to direct the reflected sound to the extreme parts. One half of the roof reflects the sound to the distant parts of the pit and the three tiers of boxes; the other half upon the upper gallery, "increasing the density of its reflection as the distance from the stage increases."

All the doors about the theatre, this author remarks, should open outwards, and the outlet should be as wide as possible, in order to prevent the bad effects arising from sudden alarms; some extra ones also should be readily opened in case of necessity. And relative to the beams and pillars being made of cast iron, he observes, "I think it would not be particularly expensive to have the whole beam and pillar-work of the theatre of cast iron: and likewise to make the elliptical part capable of being completely cut off from any fire in the other part of the building, by a jointed sheet iron curtain, to close up the stage every night after performance, or in case of fire, during the play hours. The name of this would be very attractive."

2. Mr. Edgeworth regards the building of a theatre as a public concern; as it is an affair which involves the lives and properties of so large a part of the community: its structure, therefore, cannot possess too great a degree of security for preventing as much as possible all apprehension of danger. Before the plan is arranged, he conceives the public should be called upon for hints towards its formation, and its principles should be openly discussed. Time should be allowed for obtaining information from every quarter; both architects and engineers should be consulted; the plan which is adopted should either be executed under the immediate direction of the proposer, or at least strictly followed; and no timber should be introduced as frame-work. The following objects he states as claiming particular attention in the construction of a theatre:—1. Security to the audience is the first and most necessary object. 2. Facility of ingress and egress. 3. Facility of seeing and hearing. 4. Convenience to the performers. 5. Space for scenes, with proper openings for machinery. 6, and lastly; Expense.

1. In order to ensure safety, timber should be avoided, and iron substituted in its stead. The covering of the roof might be made of copper, and the frame-work of iron; and no plumber's work be permitted to be done upon it. The substitution of iron would not increase the expense if care was taken to form the work upon the mechanical principles of strength and lightness.

Hollow bricks are recommended for floorings on account of their lightness. Those kinds of wood that are least inflammable should be adopted; and a wash might be used to render it less

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susceptible of inflammation. The painting might be finished with a coat in distemper, which could be renewed without the disagreeable smell attending a fresh painting. It is conceived that the most convenient mode of heating the private apartments would be by steam; and that the boiler should be adapted to work a steam-engine for the purpose of supplying and throwing water to whatever part of the building it was wanted.

2. For the convenience of entering a theatre and departing from it, the avenues should be rendered as wide and commodious as possible; and where private property prevents the entrances from being large, this writer thinks that this evil would be completely removed, "if the whole building were raised upon arches of a height sufficient to admit carriages; and if numerous flights of stairs were constructed within the piers which support these arches, the audience might depart commodiously in different directions, without confusion or delay. The colonnades, formed by pillars properly disposed, would permit alternate rows of carriages. Company might descend from the boxes almost immediately into their carriages: passages for those who were on foot might be railed off, and rendered secure." The expense, he thinks, might be saved by omitting the upper gallery, or by tickets for the admission of carriages under the *piazzas*.

3. With respect to the facility of seeing and hearing, Mr. Edgeworth thinks little can be said on the former that is not obvious to common sense; but that the latter might be improved by a construction agreeing with the known laws of acoustics. The audience department, he says, should be smaller and lower than it was at Drury Lane. The area of the stage might be as large as before, but it should be capable of being easily contracted; and the wings should have leaves or flaps hinged to them to close part of the openings and confine the voice. The ceiling of the stage should be made of well-varnished iron or copper, turning upon centres, like Venetian window-blinds, by which the opening of the ceiling might be contracted and the voice directed towards the audience. A gentle current of air flowing from the stage would also be of considerable service. Brazen vessels placed under the seats, or earthen jars behind the auditor, are said to increase the sound. And Mr. E. thinks the backs of the boxes might be pannelled with thin elastic plates of brass. He recommends the use of iron instead of wood wherever it could be adopted, both as a principle of economy and as a security against fire.

4. The performers should be accommodated with all possible conveniences; and rendered capable of enjoying what is understood by the English word *comfort*, which has a great tendency to promote independence and morality. Pipes to speak through should be laid from the green-room to each apartment. 5. On

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this head Mr. E. has not advanced any thing. 6. Any small additional expense that might be incurred by real improvements would be amply defrayed by a small addition to the price of tickets, which would be readily submitted to by the liberality of the British public.

Observations.—The disastrous circumstances which have recently happened to the theatres of Drury Lane and Covent Garden, have rendered the rebuilding of them a matter of public curiosity and concern: and we think with these two writers, that as this is an act which involves the safety and comfort of so many individuals, the public ought to be made acquainted with the plans, and fully apprized of the precautions which are to be adopted, in order to prevent a return of similar events, before they are carried into execution. These two letters contain several useful hints, and we doubt not but a public discussion of the subject would have produced many more; and some of them might have contributed to unite the beautiful with the useful, and thus advanced the object of rendering the whole as complete as possible. We conceive that the elliptical plan of Sir George Cayley possesses considerable advantages, yet there may be some reason to apprehend that the sound would be reflected too much to the opposite focus to that which was occupied by the stage. Muschenbroek mentions a hall in the castle of Cleves, in which a person standing in a certain place, and pronouncing a few words with a low voice, was heard by another person at a particular place only. He has not stated, however, whether this hall be elliptical or not. As it is necessary to have the principal theatres in this metropolis larger than the direct voice can fill with ease, it is a matter of considerable importance to make the best use of the reflection or repercussion of sound, which should certainly be attended to in buildings of this nature. Here we may, perhaps, be permitted to remark, that although echoes are so well known, the manner in which they are produced is not devoid of obscurity. Most philosophers have ascribed the formation of echoes to a reflection of sound, similar to that experienced by light when it falls on a polished body. But D'Alembert observes this explanation is not correct; for if it were, a polished surface would be necessary to the production of an echo; which is well known not to be the case. Echoes are frequently heard opposite to old walls, which are far from being polished; near large masses of rock, and in the neighbourhood of forests, and even of clouds. This reflection of sound, therefore, is not of the same nature as that of light. It is evident, however, that the formation of an echo can be ascribed only to the *repercussion* of sound: for echoes are never heard but when sound is intercepted, and made to rebound by one or more obstacles. That the direct sound may be strengthened by that

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which is reflected and no echo be produced, the interval between their impulses on the ear should not exceed $\frac{1}{12}$ of a second. Cloths of all kinds, and particularly woollen cloths, being very prejudicial to sound by their absorption, the circumstance of the seats rising so little above each other in the late theatres was a defect which would be remedied by their successive elevations being increased. Wood is sonorous, conductive, and vibrative; of all materials it produces a tone the most agreeable and melodious: and it is therefore the fittest for lining rooms and theatres. Paint is generally deemed unfavourable to sound from its being so to musical instruments, the effects of which it entirely destroys: this consequence does not result from the use of varnish. Water however is regarded as the best conductor; and it is worthy of remark, that when a canal was made under the pit floor of the theatre of Argentino, at Rome, a surprising difference was observed; the voice has since been heard very distinctly at the end where it was scarcely distinguishable before. It should also be observed, that in this part the canal is covered with a brick arch, over which there is a quantity of earth, and the timber floor.

The introduction of iron instead of timber for pillars, beams, roof, and wherever it can be conveniently used in buildings of this kind, we think, cannot be too strongly recommended.

An Addition Table, with a Multiplication Table, on a new Plan. By Mr. Charles Hayter.—*Phil. Journ. No.* 99.

Each of these tables consists of a series of examples in the rule to which it belongs, with their answers subjoined in the usual manner. These are arranged in horizontal rows one below another, each succeeding row being diminished by one example, until the last row contains one only.

The Editor of the Phil. Journal, after remarking how few persons there are who can add with rapidity and certainty, observes that this author's addition table, if well fixed in the memory, seems to offer a remedy for this evil. He also adds, "that the square multiplication table has the advantage of giving products readily when the fractor is the largest of two numbers; and thinks that Mr. Hayter's tables would both be improved if they were completed in the square form.

Observations.—This author's addition table may doubtless be of some service in attaining the power of adding with correctness and facility; but we deem experience only to be adequate to the removal of the contrary evil. We fully agree with Mr. Nicholson in approving of complete tables instead of such as are here given. But we must enter our protest against the uncouth expression of

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three times three *make* nine, instead of *are* nine, which is adopted in these tables, and taught in so many of our common schools; and also against the practice of designating *all* questions by the incorrect appellation of *sums*, as Mr. Hayter has done.

On the Substitution of Iron for Mahogany, and other expensive Kinds of Wood, in Articles of Furniture, and for other Purposes. By Mr. B. Cook.—*Phil. Journ. No.* 99.

Mr. Cook first introduces some general remarks relative to the expensiveness of mahogany and some other foreign woods, of which numerous articles of our furniture are made; to the abundance of iron which Great Britain produces; and to the necessity there is, in the present state of our political relation with other nations, to render this country as independent of all others as possible, by substituting native materials in the manufacture of our articles of constant use for those of foreign nature; and adds, "by so doing we should be gaining more real wealth than by the fluctuations and hazards of commerce."

The first article of furniture for which Mr. C. proposes the substitution specified in the title, is that of bedsteads. Of these he observes, "The posts or pillars, as well as the frame itself, might be cast hollow, beautifully wreathed up the posts with flowers, festoons, or clusters of fruit, or embossed with numberless fanciful ornaments, which the workman might touch up with his carver and chisel, to clear the foliage, &c. from the sand, and to make the flowers sharp and neat before they go to the finisher. The painter might colour them so as to have a more elegant and more handsome appearance than it is possible to give to carved wood; and besides, they might be cast so light, and in such chaste symmetry, as cannot be accomplished in wood." Articles of this kind, he apprehends, might be afforded at a much less price than those of carved mahogany are now sold for. Chests of drawers, bookcases, and bureaus, might also be made of iron. After the frames had been made according to any shape at pleasure, the pannels might be cut out of sheet iron to suit them, and the whole screwed firmly together when finished. Light iron frames filled up with wire-work, and lined with silk, cloth, paper, or any other suitable material, might constitute the drawers, and would diminish the consumption of the kinds of wood now in use for that purpose. Such pieces of furniture, it is stated, would not be heavier than those of wood, and when elegantly japanned, painted, and polished, would be more beautiful, and would be a security to the property contained in them in case of any accident by fire. Another circumstance which is considered as a great recommendation of this substitution, is the convenience

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it would afford in case of removal. By unscrewing the parts they might be taken in pieces, and put together again at a small expense, and the injury avoided which a large and valuable piece of furniture frequently suffers by being removed.

Mr. C. then observes generally, "Indeed, there is great scope here for ingenuity and improvement; it would give employ to vast numbers of men; it would consume our own produce, that is, iron: we should get an article that would endure for ages; and we should in part be independent of any other country, for the material that forms a beautiful and useful ornament in our houses." He then enters into some further particulars respecting the utility of this substitution for the doors, shelves, &c. of libraries, and the advantages that would result from it in saving their valuable contents from conflagration; and also for the purpose of doors in general, which he conceives might be made light and elegant, and to last for centuries.

Observations.—We agree with Mr. Cook, that the present exorbitant price of mahogany renders any substitute for diminishing its consumption a very desirable object. Iron, from its nature and abundance, appears to be the most natural and probable substitute; but we must confess that our hopes of its general adoption are much less sanguine than those of Mr. Cook seem to be. Public taste will naturally require a considerable time to accommodate itself to any thing so different from that to which it has so long been accustomed, and by which, perhaps, its prejudices have been fostered. For bedsteads, and perhaps some other articles, the substitution might be made with advantage, and we doubt not with success: but we conceive that its property of conducting heat so readily would not render it so good a security against fire as Mr. C. apprehends. Nor do we think Mr. C. quite correct if he mean to assert that "it would give employ to vast numbers of men," more than are at present employed in the furniture business; for those already employed in the cabinet manufacture would be proportionally diminished; and when the articles of iron were once made, they "would endure for ages;" and therefore, would not so frequently want renewing. A purpose for which iron might perhaps be substituted to a greater extent than in that of furniture, is in building. If those parts of a building that require large pieces of timber, as beams, rafters, joists, and a variety of supports, &c. were made of iron cast hollow, it would greatly decrease the consumption of that expensive article.

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Account of a Well for preserving and filtrating Rain-water for domestic Purposes, where a Supply of Spring-water was not easily to be obtained. Communicated by J. R. Gowen, *Esq.*—*Phil. Jour. No.* 100.

In order to effect the purpose set forth in the title of this article, two wells were sunk within a short distance of each other; each of them was 30 feet deep, and 4 feet in diameter. They were both carefully clayed, to prevent percolation, and lined with bricks in the usual manner. A communication was formed between them by means of a leaden pipe, two feet from the bottom. The water was received into one of the wells; and an oak floor full of small holes, was laid just above the communicating pipe in the other. Upon this floor was laid a stratum of well-washed coarse gravel; next, one of a finer kind; to this succeeded one of coarse sand; and lastly, one of fine sand: the thickness of the whole was about two feet. The water which is received into this well through the pipe, filtrates by ascent through the strata of siliceous substances; and the bottoms of both wells, below the pipe, serve as repositories for the sediment. The pump is fixed in the filtrating well, and both are covered; but a sufficiency of air is admitted through proper apertures.

The advantages which result from this mode of filtration, Mr. Gowen observes, are, that it is performed by ascent, and a competent space is left beneath the apparatus. "The interstices of the sand are thus never clogged, and its power is preserved unimpaired, for an indefinite period. The well fully answers the intended purpose, and the water is altogether excellent."

Observations.—There are many places even in this country, where a supply of spring-water for domestic uses, is difficult to be obtained, either from the great depths at which it is found, or the distance from which it is to be conveyed. In situations of this kind, a method of preserving rain-water for any length of time, would no doubt be esteemed of considerable importance: and we freely recommend the one described above, as the best with which we are acquainted. The mode of filtration by ascent, certainly deserves to be preferred to that in a contrary direction; and the circumstance of the sediment remaining below the floor and filtrating apparatus, prevents it from being disturbed by the pump, and mixed again with the water.

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On the Conversion of French Weights into English, in a Letter from Mr. JOHN FAREY.—*Phil. Jour. No.* 100.

MR. Farey's object in this letter, is the correction of some numbers that have been given, for the purpose of more readily converting French weights into English, and the contrary. The numbers in the two columns of the table, given in vol. i. p. 332, of the Phil. Jour. 4to. for the former purpose; and at p. 284 of vol. ii. for the latter, Mr. F. states to be erroneous, on account of the *gramme* being taken as equal to 22.966 English grains, instead of 15.475. The completion of the meridian measurement in France, however, has reduced this number to 15.444 grains English.

Mr. Farey's next remarks on this subject relate to the article WEIGHT in the last part of the *British Cyclopædia*, where it is stated, that one pound troy is 283 scruples, instead of 288: that one scruple is 1.395 grammes, instead of 1.295; one ounce avoidupois is 28.32 grammes, instead of 28.328; one dram is 37.975 grains, instead of 27.34375; and one dram is likewise stated at 1.81 grammes, instead of 1.7705, as it ought to be. The English pound troy is equal to 7021 Paris grains, nearly; but in the article referred to it is stated at 702. The English pound avoirdupois is 8532.5 French grains, and not 8538; one French grain is 0.8204 English grains; not 0.8203; and 10000 French grains are 8204, not 8233 English grains, as there stated.

Observations.—Those persons who possess the articles to which these corrections relate, will doubtless thank Mr. Farey for his information on the subject; and we doubt not that he was much gratified by the discovery of these errors, as it afforded him an opportunity of sending a letter to a publication to which he did not usually communicate. We must, however, observe, that a more correct series of tables had long before been copied into that work, from the Journals of the Royal Institution (vol. ii. of Nich. 8vo. p. 244): and we should have thought that a reference to this volume might, in a great measure, have superseded the necessity of publishing Mr. Farey's remarks. The difference between .8203 and .8204 is less than the error to which the best experiments are liable.

Plan for preventing or suppressing Fires. In a Letter from a Correspondent.—Phil. Jour. No. 102.

IN this plan, a large building is supposed to be nominally divided into different parts, and each of these to be under the care of a watchman. A small room, distinct from these, in some convenient part of the building, should be set apart for a person who

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shall have the superintendence and direction of the watchmen. This may generally be effected without leaving the room; and for this purpose, each part is to be furnished with a clock of a particular construction, fixed in the apartment of the inspector, and communicating with its section of the building, by means of cranks or wires, in such a manner, that the proper watchman may make a visible alteration on the clock at any particular moments agreed upon for that purpose, but not at any other time. Clocks of this kind, the writer says, are partially in use; and he observes, "this clock shews the usual division of time, and has also a revolving frame, in which pins are placed in sockets capable of being pressed down at particular times only, as above stated. Thus, by the use of this clock, a watchman's vigilance or neglect may be proved by the evidence of the clock itself." In large public buildings, where several watchmen and a superintendent are requisite, it is supposed that he who has this last duty to perform, should himself be watched with great care. This might be carried into effect by having a clock of the same construction, placed on the outside of the building, and immediately under the inspection of the police, or any other superintendence most satisfactory to the parties interested in the security. Hence, in case of fire, speedy assistance would be ensured; as "a discovery would not only soon take place, but personal assistance would be on the spot, and, with proper access to water at all times ensured, with the best means of applying it, an increase of the first evil would almost certainly be prevented, until additional assistance could be procured: and alarm-bells, or other signals, by the sound or character of which the particular building might be made known to firemen, could, if necessary, be instantly sounded or displayed, and a constant influx of proper persons would take place in the very infancy of danger." In order to do away the idea of great expense, which might be suggested by such a plan, it is added, that extensive and valuable buildings "have been watched for several years by a single watchman, checked by this clock alone, and with extremely few evidences of neglect." The objection, however, to a single watchman is, that his neglect is not detected till morning, while the hours of greatest danger are left to his discretion, or fear of punishment. The proposer of this plan does not consider the expense as an object when compared with the security to be obtained by it.

Observations.—When we reflect on the fires that so frequently happen in this metropolis, and the numerous depredations that are nightly committed here, the amount of which has been estimated at more than two millions annually, we cannot but lament the inefficacy of the present mode of *night-watching*, and welcome any invention which has a tendency to improve it; while we have to regret, that so little has yet been done, notwithstanding that inventions for this purpose have been made public seve-

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ral years. Samuel Day, Esq. of Hinton in Somersetshire, obtained a patent for an instrument of this nature, the specification of which was published in the third volume of the Repertory of Arts, Second Series; and which he denominates a *Watchman's Noctuary*, or *Labourer's Register*. The Marquis of Exeter, more than five years ago, informed the public, through the medium of Nicholson's Journal, that Messrs. Boulton and Watt, of Birmingham, had invented a clock for this purpose, the expense of which was only thirty shillings. He also stated, that he had then had two of them at Burleigh Hall more than four years. As the instrument is simple, and his description brief, we shall present our readers with a short extract: "They go eight days, and have a face like a clock, but do not strike. The dial goes round, and the hour finger is fixed: round the edge of the dial are moveable pins, corresponding with the quarters in each hour. A small hammer placed behind the hour finger, pushes into the dial one of the pins which happens to be under it at the time, which pin remains so abased until the dial returns nearly to the same place, when, by an inclosed plane, the pin is raised up into its first position. This gives time to have the machine examined in the morning, to see how many pins have been struck, and at what time they were pushed downwards. The hammer is moved by the pulling of a chain with a handle, like house-door bells, which, by cranks and wires, is attached to it." Register clocks for this purpose have also been indicated by Mr. Day, in the fifth volume of the Philosophical Journal.

Since remedies of this kind for some of the great evils attached to this branch of the police, have thus been publicly pointed out, we cannot refrain from thinking that their not having been generally adopted, confers some degree of culpability on those persons who have had the management of affairs of this kind; either for not making themselves sufficiently acquainted with the subject, or for not exerting themselves in carrying it into effect for the public good. The hints in the present paper are worthy of consideration; and we conceive that the expense of carrying them into effect, might be saved in the article of insurance. This mode might also be extended with great advantage to watching in general. After a description of the first of these machines above referred to, we meet with the following remarks on this subject in the second volume of Dr. Gregory's Mechanics: "One such instrument as this being placed at each end of a watchman's round, it will be ascertained how the man continued his movements through the night, to a nicety of ten minutes (or less if required), at any period of the watch; and the slightest irregularity or omission will be detected the next morning, by the person whose office it shall be to open the machine. No fraud or trick on the watchman's part can counteract the movement of the horizontal wheel, comprising the cells into which the tokens

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are to be dropped; each cell is, by this contrivance, like time itself, irrevocable when past: the watchman has no command over it, and the whole will be a kind of speaking witness of his diligence and fidelity in going his rounds, answering the next morning to the exact periods he either was or ought to have been there. By this means the calls of the watchman, which were only instituted for the purpose of his giving notice of his being on his duty, will be superseded; and a considerable expense of animal exertion will be saved to the individual, which might better be converted into that of going his rounds twice where he now only goes once. Warnings to the nightly thief of timely attack or retreat will likewise be taken away; and if, instead of an open, the watchman were to carry a dark lantern, the robber would have no security whatever in calculating the moment of his depredation, and might be detected in the very outset of his attack, as the slightest sound would alarm the watchman walking in silence, and not drowning distant noise by that of his own voice."

The chief objection to this new mode, is the expense of the time-pieces, as the number required for most parishes would be considerable. This, however, has been calculated not to exceed *three-pence* in the pound, of a rate on houses, and might, at least in a great measure, be saved by reducing the number of watchmen. The first expense also would be nearly the whole; and this would certainly be very trifling when compared with the losses it might prevent the public from sustaining. If we could contribute to the diminution of these by pressing either this or any other plan which may appear to be well calculated for the purpose, upon the serious attention of those who have the management of this branch of the police, we should feel much gratified at having been instrumental in the attainment of a very desirable object.

On the Method of taking Transit Observations. In a Letter from a Correspondent. With a Reply, by Mr. NICHOLSON.—*Phil. Jour. No.* 102.

THE object of this gentleman's letter is to inquire in what manner the fractional parts of seconds can be obtained in observations of this nature. He remarks, "In observations made at Greenwich, I observe, the time of a star's passing the meridian, is always expressed to the hundredth part of a second. How this extreme precision is obtained, as I am at a loss to conjecture, I shall be obliged to you, or any of your correspondents, to inform me."

Mr. Nicholson, in his reply, states, that it is not difficult to observe to tenth parts of a second, as any person may convince himself, with trials of a watch of five beats in that time. With regard to hundredth parts, some expedients may be devised for ob-

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serving them; but astronomical tables do not imply that these are put in practice. The results there given, are generally obtained by taking the mean of a number of observations; "and the second decimal may be considered as indicating the precise value of the first, instead of the sign + or —, which is sometimes annexed for the like purpose."

Observations.—Has the last member of this sentence any meaning? If it has, we have not yet perceived it. How the second decimal can indicate "the precise value of the first," we know not; and that this should be denoted by the sign + or — annexed, is to us a new doctrine; as, wherever we have seen those signs used, it was merely to signify whether the numbers were below or above the truth, and *not* to indicate their "precise value."

Improvement on the Spirit Level. Communicated by a Correspondent, in a Letter to the Editors.—Repertory of Arts, No. 85, Second Series.

The object of this communication is to point out a mode of ascertaining with facility and correctness, when the air-bubble rests in the middle of the tube; and the means suggested for this purpose is to have the upper side of the tube, which contains the liquid, divided into small equal parts, in the manner of a scale, and numbered each way from the middle point. This writer conceives that it would not be difficult to ascertain, when the extremities of the air-bubble rested at equal divisions of the scale, and that a greater degree of accuracy would be obtained, than is the result of the present method.

Observations.—We neither doubt the utility of the means here pointed out, nor the intentions of this writer; but we must inform him of what he seems not to have been "aware," viz. that Mr. Troughton, and perhaps some other of our artists, have made instruments of this kind for several years; and that it is the brass which contains the tube, not the *tube* itself, which is graduated. We have also seen drawings of a similar mode of constructing the level in French works. Levels of this kind, however, are far from being as general in this country, as their construction appears to deserve; and therefore, if this communication contribute, in any degree, to recommend them to a more general adoption, it will not be void of utility, though it is not new.

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Description of an improved Cock for drawing off two Kinds of Liquids. Invented by Mr. JAMES PHŒNIX, *of Liverpool. Communicated by the Inventor.—Repertory of Arts, No. 85, Second Series.*

THIS improved cock is intended to be used for drawing off two different kinds of liquids or gasses, which are required to be brought together without being connected, and which will not be injured by passing over a small space in common with each other. It consists of a hollow cylinder or tube, through the middle of which, and perpendicular to its axis, a hollow conical plug is inserted, so as to cut off the communication between its two extremities at pleasure. This plug is penetrated in one side, and when this opening corresponds with the hollow of the tube, the liquid is permitted to enter it, and thence issues at its lower extremity. By turning the plug half round, a communication is formed with the other end of the tube, and a second liquid may enter the aperture, while the former communication is stopped, and so on alternately.

Observations.—The construction of this cock is very simple, and we do not perceive any thing that will prevent it from answering the purpose for which it was intended.

On the present Mode of finding the Rates of Timekeepers. By a Correspondent.—Phil. Mag. No. 133.

AFTER some remarks on the importance of chronometers considered in a nautical point of view; relative to the reward and encouragement held out by the British Parliament, and the efforts that have been made by ingenious individuals, for their improvement; respecting the fate of the Marquis of Worcester's Century of Inventions, and the small degree of encouragement given to any of the abstruse sciences in this country; this writer observes, that when so much accuracy has been attained in practical astronomy, it may appear rather extraordinary, that the mode of ascertaining and applying the rate of a timekeeper in observations of this nature, should be called in question. He then states, that "the object intended by obtaining a rate, is to predict, how far from the truth the chronometer will be at the end of a given time; and to ascertain the degree of dependence that can be placed upon it, during the intermediate part of that time: or to find whether the quantity of error increases uniformly and regularly, in proportion as the time increases." In the present mode of trying timekeepers, this object is only partially attained, while another of no less im-

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portance, viz. the alteration of the rate, produced by different degrees of temperature, has been altogether neglected. Can it be reasonably supposed, that a machine of this nature will maintain the same rate at the temperature of 100 degrees, as it was found to have at 30°, or preserve the same in this last, that it was known to possess in the first? This writer thinks, that if the change of rate which arises from a variation of temperature, were applied as a correction to the computed rate, an essential defect in the going of watches would be obviated, and that they would be found to go much nearer the truth than is generally believed.

The author of this article conceives, that little more can be expected in the accuracy of determining the longitude at sea, but from the united efforts of art and science. He, therefore, thinks the establishment of a public observatory, where timepieces might be tried, and their rates of going registered, would be of essential service, both to the public and individuals. From this place, captains of vessels, and others interested in the most accurate measures of time, might be sure of purchasing the best chronometers; the maker would have the advantage of carrying his work to a greater degree of perfection than at present, while ingenuity and merit would be deservedly held up to public favour.

Observations.—If this be a correct statement of the present mode of finding the rates of timekeepers, and we have no reason to doubt this writer's acquaintance with the subject, it certainly is not calculated to afford that precision which constitutes so desirable an object to a commercial nation like great Britain. All calculations of this kind which are made independently of variations in the rate, arising from changes of temperature, are of little value, and, if depended upon, may in some cases lead to considerable error. We fully agree with this writer respecting both the necessity and importance of a correction, as well as in the plan which he has proposed for effecting it; as we doubt not that it would be considered a public benefit, if conducted with judgment. It would, however, be an improvement upon this writer's plan to have the timekeepers subjected to an artificial temperature as well as to that which is natural to the climate. They might thus be tried in both the extremes of temperature corresponding to the hottest and coldest climates of the habitable globe; and the transitions from the one to the other might be made either at once or by degrees, as an experienced director might deem necessary.

One of the advantages attending such an establishment, and, in our opinion, by no means the least, would evidently be its tendency to introduce deserving individuals to public notice, and to obtain them some reward for their ingenuity and skill, for want of which, their talents are at present too frequently lost to the

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country, and themselves involved in indigence and distress. Upon the whole then, as we consider the expense, trifling in comparison with the utility that might, and, in all probability, would, result from an institution like this, we do not hesitate to press the subject upon the *serious* attention of all persons who are interested in obtaining accurate measures of time.

Arithmetic applied to Signals. By PAUL LAMANON, *Lieutenant of the Navy.—Jour. de Phys. July* 1807.

THE object of persons employed with the nautical telegraph is to express the greatest number of articles with the least number of signs or signals. And for the attainment of this end, the articles ought to be digested with precision, and classed in such a manner as to prevent any mistake in the search for any order to be given, or any question to be asked.

Any character of agreement which represents ciphers, letters, or phrases, this author terms a *sign*; the application of one or more signs, a *signal*; the collection of distinct signs which are necessary in making signals, a *suite or set of flags*; and the order in which the signs are arranged, a *series*. The colours white, red, blue, and yellow, presented one by one, two by two, &c. and distributed with judgment, constitute a sufficient number of characters to express all necessary articles.

The signs being numbered and arranged in the order of the numbers in proper cases on the poop of the vessel, those which are necessary to compose a signal are readily obtained; and the articles of information to be communicated being also numbered in the same manner, the art of expressing them becomes that of expressing numbers, or any combinations of letters, words, or phrases whatever, by different combinations or assemblages of colours. Signals of manœuvre to a fleet are the most frequent of any, and the rapidity with which they ought to be executed and understood, exceeds any tachygraphical means. It becomes, therefore, highly convenient not to divide a communication into words or letters for the purpose of expressing its elements one by one, but to signify the whole at once. Advice, demands, questions, orders of police and manœuvre, &c. being formed into articles written in the books of tactics, and numbered, their expression represented numerically is invariable, and each is capable of being expressed by a single signal, by displaying the signs that denote the corresponding numbers. M. Lamanon has explained two methods for this purpose, which apply to any language, art, or science whatever, respecting which it may be necessary to maintain a conversation, or make communications, by means of signals.

The first of these methods consists in the application of the de-

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cimal numeration to signals, in which one flag expresses 0, another 1, a third 2, a fourth 3, and so on in succession as far as the tenth which denotes 9. The different flags hoisted to the head of the mast, or to the extremity of the yards, considered from bottom to top, in their vertical position, represent the ciphers 0, 1, 2, 3, &c. written horizontally from the right hand towards the left.

Thus the number 45, for example, will be expressed by the flag 4 above the flag 5; the number 54, by the flags $\begin{cases}5\\4\end{cases}$. If we have flags $\begin{cases}9\\8\end{cases}$ they will express 89; $\begin{cases}9\\9\end{cases}$ expresses 99. From this example it appears, that in order to denote any number consisting of two equal ciphers, it is requisite to have two suites of flags. If we had this combination of flags $\begin{cases}5\\2,\\3\end{cases}$ it would represent the number 523; 325 would be denoted by $\begin{cases}3\\2;\\5\end{cases}$ 888 by $\begin{cases}8\\8,\\8\end{cases}$ &c. The expression of numbers of three equal ciphers requires three sets of flags, and of four ciphers four sets, &c.; so that in order to express all numbers from 1 to 99999, it would be necessary to have five sets of flags; that is, fifty flags. By means of equivalents, references, synonymes, and complements, the author considerably extends this part of his theory.

The second method consists in the construction and use of tables. Let the figures 1, 2, 3, &c. of the following tables represent the combinations, 1 by 1, 2 by 2, 3 by 3. The Roman numerals each a flag of a distinct colour; the numbers 1, 2, 3, 4, represent each an article. Then

Table first.

I	II	III	IV	V	VI
1	2	3	4	5	6

Each number of this table is denoted by that which corresponds to it; thus, 1=I. 2=II. 3=III. &c.

Table second.

	I	II	III	IV	V	VI
I	1	7	13	19	25	31
II	2	8	14	20	26	32
III	3	9	15	21	27	33
IV	4	10	16	22	28	34
V	5	11	17	23	29	35
VI	6	12	18	24	30	36

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The numbers in this table are represented by the Roman letters which correspond to them: thus, $1=\frac{I}{I}$, $2=\frac{I}{II}$, $7=\frac{II}{I}$; and reciprocally, the flags $\frac{IV}{V}$ design the number 23, and $\frac{VI}{IV}$ the number 34.

Table third.

	I	II	III	IV	V	VI
I	1 . 2	37 . 38	73 . 74	109 . 110	145 . 146	181 . 182
	3 . 4	39 . 40	75 . 76	111 . 112	147 . 148	183 . 184
	5 . 6	41 . 42	77 . 78	113 . 114	149 . 150	185 . 186
II	7 . 8	43 . 44	79 . 80	115 . 116	151 . 152	187 . 188
	9 . 10	45 . 46	81 . 82	117 . 118	153 . 154	189 . 190
	11 . 12	47 . 48	83 . 84	119 . 120	155 . 156	191 . 192
III	13 . 14	49 . 50	85 . 86	121 . 122	157 . 158	193 . 194
	15 . 16	51 . 52	87 . 88	123 . 124	159 . 160	195 . 196
	17 . 18	53 . 54	89 . 90	125 . 126	161 . 162	197 . 198
IV	19 . 20	55 . 56	91 . 92	127 . 128	163 . 164	199 . 200
	21 . 22	57 . 58	93 . 94	129 . 130	165 . 166	201 . 202
	23 . 24	59 . 60	95 . 96	131 . 132	167 . 168	203 . 204
V	25 . 26	61 . 62	97 . 98	133 . 134	169 . 170	205 . 206
	27 . 28	63 . 64	99 . 100	135 . 136	171 . 172	207 . 208
	29 . 30	65 . 66	101 . 102	137 . 138	173 . 174	209 . 210
VI	31 . 32	67 . 68	103 . 104	139 . 140	175 . 176	211 . 212
	33 . 34	69 . 70	105 . 106	141 . 142	177 . 178	213 . 214
	35 . 36	71 . 72	107 . 108	143 . 144	179 . 180	215 . 216

The numbers written in each square of this table are expressed by three flags, hoisted perpendiculasly above each other. The two upper ones denote the situation of the square, and the bottom one the number in it; so that each square is susceptible of six different positions. The first appertaining to the flag I, the second to II, the third to the flag III, &c.; hence $1=\frac{I}{I}$, $2=\frac{I}{II}$, $3=\frac{I}{III}$, $4=\frac{I}{IV}$, &c. The number 94 will be represented by the three flags IV, and that of 210 by $\frac{VI}{IV}$, &c.

M. Lamanon then pursues this subject through the use of three other tables of different constructions, and introduces equivalents, identicals, and series; and concludes by observing, that in the tabular system we may express numbers which require the combinations of four or five ciphers. The table is preferable when the combinations, 1 by 1, 2 by 2, and 3 by 3, only are necessary, and the algorithm, when they are required, 4 by 4,

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and 5 by 5. We may, therefore, employ the table for naval signals, and the other method for those of dialogue.

Observations.—We have only prepared an account of the first parts of the two modes of communication by signal pointed out by M. Lamanon; the remainder of each indicates considerable ingenuity, but is much too abstruse and complicated to be of any practical utility. One grand objection to the whole scheme is, its want of generality, arising from the necessity of having the articles to be communicated previously agreed upon. It is easy to conceive that numerous instances might occur, when either intelligence was to be imparted, or information solicited; but which could neither be foreseen nor provided for. Hence it is that any real improvement in the telegraphic art must consist in facilitating communication by means of the general elements of language.

On the Method of communicating Magnetism without either a natural or an artificial Magnet. By M. Leopold Vacca.—*Journ. de Physique, July* 1807.

La Hire supposed that all the phenomena of the mariner's compass depended upon a great mass of loadstone which he imagined to exist in the bowels of the earth. Having taken a loadstone of an irregular figure, and enveloped it with an earthy paste until is resembled the globe we inhabit, he marked on it the poles, the meridians, and the equator, and observed that a small magnetic needle had, upon this spheroid, the direction of the meridian, with both declination and inclination, similar to the compass on the surface of the earth. This interesting experiment affords a key to some facts which appear surprising, and enables us to conceive why the magnetic inclination and declination are variable; they ought to correspond to the changes which take place in the interior of the earth, and which are frequent and irregular. It appears, then, that there is in the globe a great magnet, or loadstone, the power of which extends to our atmosphere, and is the cause of all the magnetic phenomena that we observe.

Under the influence of this idea, M. Vacca examined the phenomena of a natural magnet, with a view of applying them to the magnetic power of the earth. He placed a bar of iron within the magnetic atmosphere of a loadstone, and it became capable of attracting a small piece of iron. He also observed that it had acquired sufficient power to communicate the magnetic influence to bars of steel. In proportion as the bar was removed from the loadstone, its attraction was weakened, and its power of communicating the magnetic influence diminished, until they

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both vanished together at a certain distance. M. Vacca likewise ascertained that the bar acquired a much less degree of magnetic power when it was placed horizontally, than when it was in a vertical position. To apply these results to the explanation of a fact which had hitherto appeared singular and inexplicable, this author took a bar of iron, about three feet long, which gave no sign of magnetism when in a horizontal position, but afforded it all at once when placed perpendicularly. This sign vanished when the bar was again placed horizontally, and re-appeared when it was brought into a vertical position. This bar he considers as placed within the power of a magnet buried in the terrestrial globe, but at such a distance from it that it is capable of affording magnetic phenomena in a vertical position only. This unveils the mystery.

From this fact, an important consequence may be drawn. Since an iron bar, situated within the atmosphere of a natural magnet, is at the same time capable of attracting iron, and communicating the magnetic virtue to a plate, or bar of steel; why cannot this latter effect be produced by a bar held perpendicularly in our atmosphere? M. Vacca made the experiment, and it succeeded perfectly. This he effected by passing the steel plate many times along the bar, always commencing at the same extremity, employing a strong pressure. When the bar of iron was in a horizontal position, the experiment did not succeed. This writer concludes by observing, "I give, in short, a very simple method, and a theory founded upon it, which appeared extremely clear from the effects, and which have not before been referred to their true cause. We are within the active sphere of a magnet buried in our globe. We feel its effects, and we augment them by the means which we oppose to its power."

Observations.—Many ages have elapsed since a magnet was generally regarded as a kind of wonder; but now observation has shewn that all those instruments of malleable iron which are constantly before our eyes, and in our habitations, are kept in a perpetual state of polar magnetism by the influence of the globe we inhabit.

The natural phenomena of magnetism present the most interesting objects of inquiry; and as the action by which they are produced is both universal and durable, and subject only to slow and gradual variations, the most extensive benefits to mankind are perpetually resulting from it. It is owing to the agency of this cause that the distant parts of the earth were made known to each other; and this is the golden chain which constitutes their mutual connexion. Every thing, therefore, which has any tendency to illustrate either its nature or its operation, is justly esteemed of considerable importance. We, however, are not aware of any thing in the present communication of M. Vacca

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which can be regarded in this light. His notion relative to the magnetism of the globe possesses no degree of novelty; as the same had been previously entertained by Dr. Gilbert, Dr. Halley, Æpinus, and others; and was generally assumed as a matter of convenience, rather than adopted as a principle of belief. In a curious paper of Dr. Halley on this subject, published in the Philosophical Transactions for 1692, he observes; "The external parts of the globe may well be considered as a shell, and the internal as a nucleus, or inner globe, included within ours, with a fluid medium between. Which having the same common centre and axis of diurnal rotation, may turn about with our earth each 24 hours; only this outer sphere having its turbinating motion, some small matter either swifter or slower than the internal ball. And a very minute difference in length of time, by many repetitions becoming sensible, the internal parts will by degrees recede from the external, and not keeping pace with each other, will appear gradually to move either to the east or west by the difference of their motions." By conceiving this nucleus to be a magnet, and the shell to be magnetic also, the Doctor thought that the two great difficulties in his former hypothesis, the number of magnetic poles, and the changes of the needle's variations, might be reasonably accounted for.

Other philosophers have imagined this influence to be exerted by magnetic mines, which they supposed to be numerous near the poles. Two other ways of explaining these phenomena have also been suggested: one of these is, the supposition that all the component elements of our globe are susceptible of the magnetic virtue, but that in most substances this susceptibility is so small as to be nearly nugatory; as it has been observed in iron only, in any eminent degree. The other mode of explanation supposes the magnetic action to be exerted by the molecule of iron imperceptibly pervading the various bodies in nature, but which, at least in many instances, have hitherto eluded the rigours of chymical analysis.

A memoir was a few years since presented to the French National Institute by *Humboldt* and *Biot*, "On the Variations of terrestrial Magnetism in different Latitudes." These philosophers are the first who have determined the position of the magnetic equator by direct observations; they have also ascertained, that the magnetic force increases in proceeding from that equator towards the poles; and they have likewise given a mathematical hypothesis, and deduce from it a formula for the inclination of the needle, which, in their opinion, affords results agreeable to observation.

The magnetic equator they have found to be a great circle of the sphere, having its plane inclined to the astronomical equator, in an angle of 10° 58′ 36″; its two nodes on this equator being at 120° 2′ 5″ west, and 59° 57′ 55″ east longitude from Paris, the

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two points where the axis of the magnetic equator penetrates the surface of the earth being situated respectively at 79° 1′ 4″ of north and south latitude, the former being in longitude 30° 2′ 5″ west from Paris, and the latter at 149° 57′ 55″ eastward from the same place. With respect to the intensity of the magnetic force, they found it vary in different latitudes; and some observed anomalies manifestly prove that its general action is sensibly modified by local circumstances, as differences are observable at places situated at small distances from each other. With a view of determining the law by which the inclination of the needle is regulated, Biot supposed the two magnetic poles of the earth to be represented by two centres of attraction, situated on the axis of the magnetic equator, and at equal distances from the centre of the earth. He then assumed the variation in the intensity of the attractive force to be reciprocally as the square of the distance, and calculated its effect on any point of the earth's surface: his results he found to approximate more and more to the truth, as the distance between the centres of attraction was diminished; and when this became nothing, or the force was considered as residing in the centre of the earth, the numbers agreed with those which had been obtained by observation in various places, in both hemispheres. In this formula, u represents the angle included between the magnetic axis and a radius, drawn from the earth's centre to any assumed point on its surface; β the angle contained between the line which coincides with the real position of the needle and the magnetic axis, and I the inclination of the needle to the horizon of the place of observation; then $\tan. \beta = \frac{\sin. 2w}{\cos. 2w + \frac{1}{3}}$; and $I = 90° + u - \beta$. M. Biot, however, considers this hypothesis only as a mathematical abstraction, which may be useful in ascertaining the existence and magnitude of future changes. It is remarkable, that M. Biot has not observed that $\tan. \beta\, I = 2 \cot. u$, which is a formula far more simple and convenient than that which he has employed. It was, probably, by means of some such formula as this, that Dr. Young constructed his diagram of the lines of equal dis. pl. 41, fig. 578.

With respect to an iron bar becoming magnetic, when placed in an upright position, it has been long known to philosophers; and one of the earliest observations of this nature, with which we are acquainted, was that of Gassendi, relative to the iron rod which supports the cross above the clock of St. John's at Aix, in Provence. Similar observations have also been made at different times, on other bars. The vertical position in which M. Vacca placed the bars in his experiment, however, is not the most favourable for their receiving the action of the globe; but that which is assumed by a needle, suspended by its centre of gravity, and turning freely in a vertical plane. This inclination at Paris is about 72 degrees: and it was remarked by Æpinus, that if a rod which is held in a favourable position for receiving the terrestrial

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magnetism, be struck a number of times with a hard substance, the effect will be increased. With regard to the circumstance of communicating the natural magnetism thus acquired to other bars, upon which M. Vacca seems to lay so much stress, M. Haüy, in the second volume of his "*Natural Philosophy*," observes, "Philosophers have availed themselves of the slight degree of magnetism a rod acquires by the simple action of the globe, to solve the curious problem, *of being able to magnetize steel bars, even to saturation, without having previously had any magnet in their hands.* All that is necessary, is to cause bars of soft iron to acquire an incipient virtue, by placing them in a proper position with respect to the magnetic meridian of the place. Those bars are then employed to magnetize others of a harder nature, which is effected by means of friction on their surfaces. These last are made to perform the same office to other bars; and so on, till the force of the bars in question is increased, even to a *maximum*." From a perusal of these observations, we apprehend, that M. Vacca's claim to either originality or improvement, will have completely vanished.

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1. *On the Alterations that have taken place in the Structure of Rocks, on the Surface of the basaltic Country in the Counties of Derry and Antrim. By* W. Richardson, *D. D.—Phil. Trans. Part II.* 1808.
2. *Remarks on the preceding Article. By Mr.* J. Farey.—*Phil. Mag. No.* 132.
3. *Observations, suggested by the above Paper of Mr. Farey. By* John Carr.—*Phil. Mag. No.* 133.

1. As this paper of Dr. Richardson's contains a minute description of a great number of particular appearances which occur in the district which has fallen under his observation, accompanied with plates, it is not susceptible of a regular abridgment; we shall, however, endeavour to give such a brief account of it, as may afford our readers an idea of his undertaking, and furnish them with his principal results.

Dr. R. first offers some general remarks relative to the stratifications of this tract of country, which comprehends the greater part of Antrim, and the east side of the county of Derry; and observes, that in many places these several strata are completely laid open for examination; and specifies Magillian Rock, Cave Hill, Bengore, and Fair Head, as being of this nature. He then enters into a minute description of the promontory of Bengore, and its stratification. The perpendicular part at the summit is 170 feet, and the precipitous part, from the bottom of the pillars to the sea, 200. This promontory consists of an assemblage of sixteen strata, each of which, Dr. R. describes, commencing with the uppermost, and

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then selects from the mass of facts which this façade exhibits, such as seem applicable to geological questions, and are likely to throw light on such subjects. These are,

"1. Each stratum preserves accurately, or very nearly, the same thickness throughout its whole extent, with very few exceptions.

"2. The upper and lower surface of each stratum preserve an exact parallelism, so long as they are covered by another stratum; but when any stratum becomes the superficial one, its upper surface is scalloped, or sloped away irregularly, while the plane forming its base, continues steady and rectilineal; but the parallelism of its planes is resumed, as soon as another stratum is placed over it.

"3. The superficial lines bounding the summit of our façades, and our surface itself, are unconnected with, and unaffected by, the arrangement of the strata below them.

"4. Nature, in the formation of her arrangements, has never acted upon an extensive scale in our basaltic area (at least on its northern side, where our continuous precipices enable us to determine the point with precision), but changes her materials, or her arrangements, or both, every two or three miles, and often at much smaller intervals.

"5. Wherever there is a change of material, as from one stratum to another in a vertical line; or where the change is in a horizontal direction by the introduction of a new system; or where a whyndyke cuts through an accumulation of strata; in all these cases, the change is *per saltum*, and not *per gradus*, the lines of demarcation always distinct, and well defined; yet the different materials pass into each other, without interrupting the solidity and continuity of the whole mass.

"6. The façades on our coast are formed, as it were, by vertical planes, cutting down, occasionally, the accumulations of our strata; the upper part of these façades is generally perpendicular, the lower steep and precipitous.

"7. The bases of our precipices commonly extend a considerable way into the sea; between the water and the foot of the precipice (and especially near the latter), there is frequently exhibited the wildest and most irregular scene of confusion, by careless observers supposed to be formed by the ruins of the precipice above, which have fallen down: such, no doubt, was Mr. Whitehurst's idea, when he describes one of these scenes as an awful wreck of the terraqueous globe! But a more attentive observer will soon discover that these capricious irregularities, whether in the form of rude cones, as at *Beagyn Daana*, and the west of *Pleskin*; or towers, as at the dyke of *Port Coean* and *Castro Lewit*, at the foot of Magilligan façade; even spires and obelisks, as to the westward of *Henbaan*, and at the *Bull of Rathlin*; yet all of these once formed part of the original mass of the coast, strati-

fied like it, and their strata still correspond in material and inclination, with those in the contiguous precipice.

"9. These vertical sections, or abruptions of our strata, are by no means confined to the steeps that line our coast; the remaining boundary of our basaltic area has several of them equally grand; and similar abruptions or sections (though not so deep), are scattered over a great part of our area, and especially on the ridges of our hills and mountains, which are cut down in many places like a stair, by the sudden abruptions of the basaltic stratum.

"9. Wherever the strata are thus suddenly cut off, whether it be a mass of accumulated, as in the façades on our coast, of solitary strata in the interior; the materials on one side of the abruption are completely carried away, without a fragment being left behind; while, on its other side, the untouched stratum remains entire and undisturbed."

Dr. R. then endeavours to trace, by the marks nature has left behind her, some of the grand operations she once executed on the surface of our globe; and in this attempt he institutes an inquiry relative to the formation of the perpendicular façades; enters into a discussion of a question, which has at all times exercised the ingenuity of geologists, viz. the cause of those inequalities with which the surface of the earth is so diversified; enumerates some remarkable inequalities in the surface of the basaltic country under consideration, which have been produced since the consolidation of its strata; and exhibits some proofs, that its strata, which are now interrupted, were once continuous. He then observes, "that conclusions which unavoidably follow from the consideration of these facts, are,

"That the hills and mountains, in the district which has been described, were not raised up or formed as they now stand, but that they are the undisturbed remains of strata that were left behind, when stupendous operations carried away the parts that were once contiguous to them.

"That the inequalities of this surface were all produced by causes acting from above, and carrying off whatever they touched, without in the least disturbing what was left behind."

2. Mr. Farey, in his observations, states that of the nine geological facts which Dr. Richardson has deduced from his examination of the basaltic area, specified in his paper, seven of them exactly correspond with his own experience in other districts; and he thinks that he is prevented from perceiving a similar correspondence in the fourth and fifth of those facts from not fully comprehending some expressions they contain. Mr. F. then observes, that the results of his observations have been fully communicated to numerous friends, "shortly alluded to in the 28th and 31st volumes of the *Phil. Mag.*; "and more fully explained under the article *Denudation*, and others, in *Dr. Rees's New Cyclopædia.*" For the purpose of shewing that *hummocks*, or insulated caps of strata, are not confined to any particular

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kind of stratum, Mr. F. presents a list of several, of different kinds, which are to be met with in separate districts which he has examined. The cause which Mr. Farey deems fully adequate for all the geological appearances with which he is acquainted (at least as far as we can comprehend him), is, that of "a *satellite*, moving near enough, and with attraction sufficient, to *reverse the direction of gravity*, for the instant of its passage over any given tract of the earth's surface." And the time at which this took place is conjectured by Mr. F. to have been "at that awful but important period, when 'God said, Let the waters be gathered together, and let the dry land appear."

3. Mr. Carr agrees with the two preceding writers, that mountains do not owe their existence to successive masses of matter piled upon each other by some means, but that they are solely indebted for it to the durability of their materials, which have resisted the effects of those powerful agents, which swept away the surrounding country in which they were imbedded; and therefore the loftiest mountain can only be regarded as the remaining speck of an extensive country which once occupied the same, or a superior level. This mode of explaining the formation of mountains he conceives to account for that of the extensive strata of the earth at the same time; as "the incalculable masses of materials, necessary to form the latter, could only be derived from the destructive transportation of other strata equally extensive; and the present elevation of stratified mountains is demonstrative evidence of the former existence of the countries, which, in disappearing, have furnished such vast masses of diversified material for the formation of other stratified countries in other situations." The chief desideratum in geology, he thinks, is a satisfactory account of the original formation of countries of this nature: as, this being once accomplished, the irregularities and anomalies in the strata themselves would be a comparatively easy task. The great source of hypothetical reasoning on this head, Mr. Carr conceives to have arisen from the mistaken notion that the present laws of nature are inadequate to the production of past effects; without considering that the natural causes which are now operating changes on our globe, have been in action millions of years, and that it is the almost infinite duration and variance of their action, rather than the apparent little which we can perceive them performing, which will enable us to account for the stupendous effects which they have accomplished:" he therefore considers Mr. Farey as preparing to add another hypothetical system to the number of those that have already been offered, and which have so much retarded the progress of real knowledge.

The only agent in nature to the action of which geological phenomena can be ascribed, with any degree of probability, is *water*. The changes which have arisen from this source, are naturally divided into two classes; those which have been produced by the

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streams which flow over the surface of countries, and those of much greater extent and importance, that have been effected by the circumfluent ocean: each of these classes he proposes to pursue in two subsequent papers.

Observations.—The two following extracts, when combined, tend to shew the influence under which these observations of Dr. Richardson were made: "I shall look to Nature alone, without much reference to opinions, and shall endeavour to trace, by the marks she has left behind her, some of the grand operations she once executed on the surface of our globe;" and he afterwards adds, "Thus, by making ourselves acquainted with effects, we shall be better qualified to investigate causes; and if those effects shall appear to be beyond the powers of such natural agents as we are already acquainted with, we shall be justified in admitting the performance of operations to which we have seen nothing similar; and also in admitting the former existence of powers of far superior energy to any we have ever known in action." Dr. R. however, does not think that the inequalities in the surface of the basaltic area he has examined, can have been produced by the agitation of superincumbent waters, and consequently does not regard them as the effects of a general deluge. We were, however, a little surprised at observing how studiously this Doctor of Divinity had avoided using the name of the Supreme Being, as the Great First Cause; and for this purpose he inserted the following note: "By the word Nature, which so frequently occurs in the course of this memoir, I always mean, according to Ray's definition, *the wisdom of God in the creation of the world*." Here the *divine* has certainly shewn himself to be much less under the influence of *true* philosophy, than the *poet*, who said,

> "Nature is but a name for an *effect*,
> Whose *cause* is GOD."

What could render Dr. R. so cautious in this respect? Was he afraid of forfeiting the reputation of being considered as a *genuine* philosopher? However this may be, if he had only thought of substituting the definition for the word itself, we are persuaded that he would not have adopted it, as the inconsistence in which it necessarily involves him, are too glaring to pass unnoticed by the most careless writer. As a fair specimen of this kind of substitution, we shall transcribe the following sentence: "As soon as we enter the basaltic area, we begin to perceive traces of these arrangements; as we advance further north, they increase, and in the tract near the shore, and especially at the island of *Rathlin*, which seems to have come fresher from the hand of (Nature) the wisdom of God in the creation of the world than the rest of our area, the stratification of the whole is perfectly visible, and the nature of the several strata laid open to us at their abrupt and precipitous terminations." The absurdity is here too manifest to

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need any comment. Such, however, are those in which authors are perpetually involved, whose spirit of philosophizing leads them to separate what Infinite Wisdom has joined together as cause and effect.

We can assure our readers that they will not lose much of the information contained in the original, through the brevity of the second part of this article, except that of being convinced how easy it is for a person to profess to write on a particular subject, and either by *design* or accident write about himself. And we have no apprehension of being accused of bringing the charge of egotism against Mr. F. too hastily, when we observe that the pleasing and important words *I*, *my*, *mine*, and *myself*, occur between forty and fifty times in this paper of seven pages. Another respect in which we are far from being satisfied with Mr. F.'s present communication is, the references it contains to different parts of different books written by himself. Would it not be more consistent with a real friend of science, to publish the outlines of his theory in some one place, in an *intelligible* form, and refer to that, and that only, than it is to drop a hint in one place, and another in a second, &c. according to his present *ambiguous* mode? Besides, who is to examine the various articles in Rees's Cyclopædia, the different volumes of the Philosophical Magazine, &c. to pick up the scattered *atoms* of what he is pleased to call a theory?

Mr. Carr's remarks are, upon the whole, judicious: it remains, however, for him to prove, in *opposition* to the *Mosaic account*, that the causes, whatever they may be, "have been in action millions of years."

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1. *An Account of the Progress and present State of the Works undertaken with a View of forming a Tunnel under the Thames.—Repertory of Arts, No. 84, Second Series.*
2. *On the intended Thames Archway between Rotherhithe and Limehouse. By Mr.* Farey.—*Phil. Mag. No.* 133.

The Thames Archway Company was established by Act of Parliament, for the purpose of forming a tunnel under the river Thames, either for foot-passengers, or carriages, or both. In order to the execution of this undertaking, it was deemed necessary to make a drift-way to the deepest part of the river, as a preparatory step; and then to commence the tunnel at this place, and proceed to both sides of the river. For the purpose, therefore, of carrying the first part of this plan into execution, a shaft was sunk on the south side, near Horseferry, Rotherhithe, and a drift-way made to the point proposed. It was then determined to continue this drift to the opposite shore, chiefly with a view of exploring the ground, and enabling the engineer to anticipate and guard against difficulties.

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After the drift had been extended 1037 feet from the shaft, some difficulties occurred in its further execution, on account of the quick sand there met with; and the engineer "then gave it as *his opinion*, that an *underground* tunnel could not be made in that line, unless the fractures were covered by caissons, without which the further progress of the drift would be useless; but that he had no doubt of being able to make a tunnel over the *same* line, through the river, sufficiently deep in its bed, by means of moveable caissons, or coffer-dams, and at a less expense considerably than the original estimate for the underground plan; and *without any impediment to the navigation of the river*. Under these circumstances, the further progress of the works was suspended."

The Directors, however, wishing to avail themselves of the best mode of completing their undertaking, and considering its novelty and importance, offered the following premiums for this purpose:—"Two hundred pounds to the person whose plan shall be adopted and acted upon; and a further sum of *three hundred pounds* if it be executed." The size of the tunnel is intended to be such as to admit two carriages to pass each other; or two tunnels of smaller dimensions, each capable of admitting one carriage. A foot tunnel is contemplated only in case a larger shall be found impracticable.

That engineers and others might be assisted in forming correct judgments of the undertaking, the Directors caused an account of such facts, relative to the nature of the ground, &c. as had been noted during the progress of the works, to be printed and circulated among the proprietors; and accompanied it with a plan and section of the works. From this account it appears, that the depth of the shaft is 76 feet 4 inches, and lined with nine inch brick-work laid in cement impervious to water.

2. Mr. Farey expresses his surprise, that after more than three years of ineffectual attempts towards constructing a tunnel under the bed of the river Thames, the Directors of this undertaking should now be *advertising* for plans for its construction; and particularly when there are so many engineers of established reputation in this country, under whose direction such extensive tunnels have been executed, and in situations where all kinds of difficulties were to be overcome. He thinks the purpose would have been much better effected by calling in the professional aid of one or more of these experienced engineers; and observes, "Surely it cannot be expected by these gentlemen, that any of the experienced engineers alluded to, will submit their designs and estimates for the great work which the proprietors have undertaken, on the terms and for the considerations held out; *who* is to decide on the merits of the different designs which may be delivered in? and *who* is to superintend and execute the design which may be adopted? On both of these questions the probability either of honour or profit to be derived from their labours,

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will in all likelihood turn, according to the conditions which the Directors have laid down."

Mr. Farey then refers to some previous suggestions of his, published in Dr. Rees's New Cyclopædia and the Philosophical Magazine, relative to boring in the bottom of the river, and the strata of which the under-ground was composed. He also gives it as his opinion, that a tunnel laid in the bed of the river ought to be preferred to one deep underground, which the Directors seem to prefer; and he predicts, that if the latter be adopted the result will be a grievous disappointment. In one of the articles which Mr. F. has referred to above, he recommended *piling off* the river for the purpose of constructing the tunnel through the water, but he now suggests "to the consideration of those engineers who may turn their attention to the subject, an immense tub in the form of a frustum of a cone, secured by hoops outside and framing inside, but without either top or bottom." When this has been fixed so as not to allow of any influx, the water is to be extracted out of it by means of engines; and a length of 70 or 80 feet of the tunnel being constructed within it, the tub is to be removed to another of similar length, and so on to the end. Mr. F. has subjoined to his paper a copy of that we have abridged in the first part of this article.

Observations.—This gentleman seems to be about as well informed on the subject of tunnels as on most others on which he is so frequently pleased to exercise his varied abilities. Before he had expressed his great surprise that such men as Rennie, Jessop, or Telford, had not been consulted or employed in this undertaking, it would have been prudent in him to have ascertained the fact; and before he ventured a positive assertion, that "*tunnels in the most difficult situations, in considerable number, have been constructed, and where difficulties of almost every kind have been overcome,*" it would have been wise in him to have pointed out where one at least existed pregnant with similar difficulties to those which must be expected to occur in passing under a deep river, and necessarily so near the bottom of it, in order to procure a short line and easy ascent, as would secure to the proprietors a revenue adequate to the capital expended. If he will tell the public where one *such tunnel* has been executed, we think he will not only surprise that public, but even those very engineers whom he has mentioned as having constructed several.

It is rather curious, that a man of Mr. F.'s research should not perceive the difference between constructing a tunnel through a hill for a wet passage, and under a river for a dry one. In the former case, to meet with water is desirable and useful, and cannot block up the works; it runs off, whatever the quantity may be, through the canal on the same or a lower level: but in the latter, an influx of water beyond the power of the engine may

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be fatal to the undertaking. It therefore necessarily requires very different management to construct a tunnel under a river from that of passing through a hill; and as we are ignorant of a tunnel having any where been made similar to the one in question, we are inclined to think the Directors of this undertaking acted wisely in appealing to, and endeavouring to obtain from the ingenuity of their country the best means of executing this important object. They did not, as Mr. Farey uncandidly insinuates, "advertise for the schemes of *inexperienced adventurers*." They offered a premium of 500*l.* which might well be supposed to attract the notice even of *some* professional men of considerable talents, though we presume Messrs. Rennie, Jessop, or Telford, might not be expected to become competitors. We understand, however, that the opinion of one of these gentlemen was taken at the beginning of the business; and as the printed particular of the works states, "that, by the unanimous opinions of every engineer who had been consulted, a drift-way was deemed necessary as a preparatory step, &c." we must presume many others had concurred in the mode adopted. Thus it should seem Mr. Farey has but little foundation for his affected surprise that the Directors did not consult high professional men, unless indeed he has been astonished, that, after his *skilful* Essay in Rees's Cyclopædia, he was not applied to: but if the Directors were so blind as not then to see his great merit as a civil engineer, how much more will *he* be surprised at their not appealing to him after this second effort of his genius in that way! He owns, that when he wrote the short notice of this undertaking "for the Cyclopædia," he did not give it that "consideration which its obvious importance has since occasioned it to have;" therefore, "instead of piling off the river, as I there hastily mentioned, (hastily mentioned in an elaborate work *intended* for permanent instruction!) I would suggest (says he) an immense tub, in the form of the frustum of a cone, secured by hoops *outside* and polygonal framing inside, &c. &c.; the bottom of it so secured to the bottom of the river as to prevent the influx of water or sand from the river after the water is extracted from the tub by engines." It is a little extraordinary that both the methods mentioned above by Mr. F. should have been (as we learn from good authority) formerly suggested by other engineers; the first, perhaps, with as little consideration as Mr. Farey, in his hasty communication to the Cyclopædia, had bestowed upon it; the latter certainly with much more, for the authors of that method (for there were more than one) did not, like Mr. Farey, propose to secure the *outside* by hoops, thinking (perhaps very unphilosophically), that when the water was extracted from the *inside*, the outside might not want the protection of hoops; for that the pressure of the water on the outside of this tub would,

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when the water was taken from within it, be very likely to diminish its diameter, and so render the hoops worse than useless—mischievous. If there had not been this and some other such *trifling* variations in the two plans, Mr. F. might have been suspected of having borrowed the thought; and indeed, a man of less inquisitive research than Mr. F. might have stumbled upon some such plan as the tub from the Directors' statement, that their engineer had proposed to make the tunnel "*by means of moveable caissons, or coffer-dams.*"

Mr. F. remarks, that it is too much to be feared that the councils which first prevailed in adopting a *deep underground* tunnel rather than one laid as near as may be to the bottom of the river, have still a prevailing influence among the Directors; in which case he ventures to predict "that the expectations of the public will be grievously disappointed." Although we have differed from Mr. Farey in every other feature of this business, both as to his facts and opinions, yet we agree with him in this sentiment. But it is in the sentiment only, that an underground plan is not likely to succeed; and not in the fact he assumes, that there exists a prevailing influence among the Directors, to adopt an *underground* plan. Where he learned that such an influence prevailed, we know not. We have heard, and we believe from correct authority, that the Board of Directors will adopt any plan which shall be deemed, on due consideration, by proper judges, the most eligible. We would ask, if he draws an inference of such intention from their printed statement, if it would not be asking too much from a gentleman, who confesses that he writes hasty observations for a Cyclopædia, to deduce correct inferences from well-considered premises? It is much easier to assume facts; and we certainly have no right to expect Mr. F. to depart from his accustomed mode of argument.

We own ourselves to be in that state *now*, in which Mr. F. *was* some time ago—"sanguine enough to expect to see several tunnels constructed under our navigable rivers;" and our reason for this opinion is, because we perceive the Directors of the present undertaking are not running precipitately hand over head, in order to effect a favourite project in a short time, but are acting as we think becomes men who are entrusted with so important a concern, endeavouring to procure the best possible information on the subject from every accessible source, before they commence operations, which will eventually decide, not only the fate of this, but, in all probability, have great influence in promoting or preventing future projects similar to it. The drift-way was a preparatory step, an exploring passage acknowledged by every engineer to be necessary. It has answered the end, it has shewn the nature of the ground, which nothing else could discover; and of course enables the skilful and cautious engineer where to expect, and how to guard against difficulties. Before the drift was made, it was only an idea, that a tunnel could be made by sub-

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terraneous means, if the ground proved favourable. The ground proving unfavourable for that method, does not prove it impracticable, but difficult; and in our opinion, clearly shews the advantages of an overground plan.

We are sorry that a gentleman who has the knack of writing so prettily on a great variety of subjects, as Mr. F., should not seize upon those, wherein his judgment is more matured, and his information more correct; and that he should so constantly indulge himself with references to, and quotations from, his own works. If not in reality, it appears at least as if the *shop* was always uppermost, for we are twice or thrice more reminded in this article of his writing for Dr. Rees's New Cyclopædia, and the Philosophical Magazine.

REVIEW OF SPECIFICATIONS OF PATENTS,

PUBLISHED IN THE REPERTORY OF ARTS, DURING THE MONTHS OF APRIL, MAY, AND JUNE, 1809.

Messrs. CLATWORTHY's *Patent for Shears on an improved Construction for shearing Sheep. Dated March* 1809.—*Repertory of Arts, No.* 85, *Second Series.*

THE improvement in these shears is in the bow, which is made double, or with a snout in its middle of about an inch or an inch and a half in length. The whole length of the bow is nine or ten inches, and when finished, the snout is twisted; its top edge lying one way, and its under edge the opposite. A rivet is sometimes put through the snout when it is weak.

Observations.—The additional length of the bow of these shears, as well as its shape, appears to be calculated to remove a part of its usual stiffness, which was frequently considered as a defect, without destroying its elasticity.

Messrs. TREVITHICK *and* DICKINSON's *Patent for Machinery for towing, driving, or forcing, and discharging Ships of their Cargoes. Dated July* 1808.—*Repertory of Arts, No.* 85, *Second Series.*

THE apparatus which these patentees make use of for effecting the objects set forth in the title of this specification, consists of a rowing wheel, resembling an undershot water-wheel, furnished with floats or pallets, which they name propelling boards: the size of the wheel, as well as of these boards, is proportionate to that of the vessel to which it is adapted; and which they term a *nautical labourer*. This wheel is fixed vertically in an air-tight

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box or casing, and with its axis at right angles to the length of the ship. The vessel should have two keels, at such a distance from each other, as to permit the wheel to turn between them; the casing is open at bottom; and the propelling board which is lowest, is even with the bottoms of the keels. The water in the case is prevented from rising to the level of the surrounding surface by means of air which is forced into it by an air-pump adapted to the purpose, and which, as well as the wheel, is worked by a steam-engine. When this vessel and apparatus are to be employed in towing others, they are fastened together and caused to proceed by the force of the steam-engine: but if strong tides or currents oppose their progress, the towing vessel is propelled forward alone to a convenient distance, and secured by means of what bargemen call sets, or an anchor: then the vessel in tow is drawn up by a windlass and rope worked by the steam-engine, which is capable of being thrown out of gear with the wheel, and put into gear with the windlass. The anchor is also weighed by the same windlass and power; and the vessels proceed thus alternately, until the stream or current be passed. The same mode of operation may also be applied for towing ships out of harbour.

The machinery which is thus employed for towing vessels, may also be used for loading and unloading them. The common blocks and tackle used for this purpose may be worked by the windlass; but when more dispatch is necessary, the business is facilitated by working a barrel instead of the windlass. On this barrel, which is kept in constant motion, as many endless belts as are necessary for the business in hand are placed. "These belts pass respectively over a pulley of two grooves centred in a lever, the lower end of which is on a hinge. To the upper end of the lever, which is the longest from the centre of the pulley, a rope is attached, which goes to the vessel then discharging, where it passes through a common block in the rigging, whence the end of it descends to a man standing on the deck at the hatchway of the ship discharging. This rope is for the purpose of enabling the man, by pulling it, to tighten the endless belt by means of the longest end of the lever." By this means, motion is communicated from the barrel to the pulley, and continued as long as the rope is kept tight. Another rope, called the discharging rope, is fastened to the other groove of the pulley, and passed through a block in the rigging of the vessel containing the apparatus, and also through another in the vessel to be unloaded, and is of sufficient length to descend into the hold. To this rope the article to be raised is fastened, and when the motion is communicated to the pulley, this rope is wound up, and the article raised as long as the man keeps the other rope tight. The article being thus raised as high as necessary, the man slackens the rope and permits the pulley to revolve the contrary way, and the end of the rope to

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descend into the hold again, when a second article is raised in a similar manner by tightening the rope held by the man, and so on. Several vessels properly arranged, may be discharged by the same apparatus at the same time: or any other common mechanical means may be employed instead of the barrel and endless belt.

These patentees state, that the novelty of their invention consists in employing a vessel and a steam-engine as a moving power, for the purposes above described. And when either towing or discharging alone is the only object to be attained, only a part of the apparatus is necessary.

Observations.—The objects which these patentees have in view, are certainly important, and the means which they have employed for obtaining them are no less ingenious. A variety of cases may occur in which vessels may be prevented from attaining their ultimate destinations, without loss of time, by local circumstances, which may be overcome by this contrivance; and a considerable number of harbours in this island might be specified in which ships are confined by the very wind that is favourable for their voyages after they are once at sea. In each of these cases the application of the present contrivance might be productive of considerable advantages. Amongst other objects to which that part of the apparatus relating to unloading vessels of their cargoes might be applied with success, that of superseding the necessity of coal-heavers is not the least. We have long wished to see this *slavish* employment dispensed with, and gladly welcome any invention which tends to that end. As several ships might be unloaded at one time, and a much greater quantity brought up at once by the method now proposed than by the present mode, and all the laborious part avoided, we conceive that it might be used with considerable advantage.

Mr. Brown's *Patent for Improvements in the Construction of a Press for printing Books and other Articles. Dated June* 1807.—*Repertory of Arts, No.* 84, *Second Series.*

This press is made of cast iron; and its principal parts are, a bed faced true for the types to rest upon; a follower which gives the pressure on the types; a screw and girdle; a bevel wheel and pinion fixed to the side of the press, with a handle to put it in motion; this wheel is fixed on a vertical spindle or shaft, the top of which contains a rigger, sheve, or cylinder, with a catch or clutch to throw it out of gear when required; a cylinder which spreads the ink over the types; and a moveable frisket, for the purpose of conveying the paper over the types. The bed or platform upon which the types are laid, slides in and out below

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the roller or cylinder which feeds the types with ink; the motion is communicated to both the platform and cylinder by turning the handle and pinion. The roller is covered with flannel or any other similar elastic substance, and then with parchment or vellum to prevent the ink from soaking too far in, and to give it a spring, and again with superfine cloth closely sheared, which receives the ink for the supply of the types. The cylinder receives the ink from a trough placed beneath it, and a smaller cylinder is made to roll on this large one, and distribute the ink over its surface: a brush, however, may be occasionally applied for this purpose. The lever of the screw is connected with the upright shaft or axis, which bears the bevel wheel, by means of a rope or strap. The frisket is attached to the end of the platform, and moves in and out beneath the screw with it.

The rollers for feeding the types, which Mr. Brown regards as his principal improvement, may also be applied to the presses now in common use, by means of a fly-wheel and traddle for putting them in motion; or they may be rolled over the types by hand, or in various other ways, sufficiently understood by every competent workman. Relative to the specific objects of the patent, Mr. B. observes, "My first improvement is on the press; my second improvement consists in my using barrels or cylinders for feeding the types with ink; and my third improvement consists in the loose frisket, and the mode of using it."

Observations.—Every invention which has a tendency to improve the general execution of this extensive and important art, deserves public encouragement, and from its great utility will, without doubt, obtain it. We, however, are inclined to think, that Mr. Brown has made more alterations than real improvements: the extent of the latter we shall leave for experience to determine, under the conviction that the utility of whatever is such, will insure it a proportionate degree of success.

Mr. Berrollas's Patent for a Method of making infallible repeating Watches. Dated October 1808.—*Repertory of Arts, No. 84, Second Series.*

The outside of this watch resembles that of common watches, except in the pendant, which is here mounted with a button, consisting of two parts: the lower of these parts does not move; the upper part turns round by means of an endless screw, and comes out to the extent of four turns. By turning this upper part to the right, it separates from the lower part, operates upon the hour rack, and is capable of being unscrewed only while the hour indicated by the index is struck. The same part being then screwed towards the left hand, causes the quarters to be struck, as the

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hours had previously been, and is again brought back to join the lower part, and the quarter rack returns to its place.

The movement of this watch is the same as that of a common watch; and the wheel-works are of the same height. The kind of escapement required is not particular. The hammer, by which the hours and quarters are struck, is the only additional piece in the frame of the movement: it is this which distinguishes the infallible repeating watches from a common watch. The motion is composed of three principal parts: the hour rack, the quarter rack, and the pendant and endless screw: but for the particular construction and mechanism of these and their collateral parts, we must refer to the plate accompanying the specification.

In order to elucidate the nature and superior utility of his invention, the patentee adds the following observations: "1. The old repeaters contain five wheels more than the common watches, besides five pinions, and a barrel and main spring, which are all necessary to put in action the motion; they also have two hammers to distinguish the hours from the quarters. Whereas the new repeater is composed of the common plain movement and wheel-work, with the addition only of a hammer, which is placed in an insulated situation, having no communication whatever with the wheel-work. 2. The old motions being so very complex, are in their nature liable to be out of order from the slightest cause, because the chain of the motion which winds the main spring of the repeating work is easily broken by the means of the pressure, its very structure, and its attendant friction; and lastly, because the action of it depends upon the main spring and wheel-work, the latter of which is apt to be disordered, and the former snaps and breaks of itself. Whereas the new motion acts in itself, and has no dependance on wheel-work, or any other piece that is subject to be broken; an endless screw sets the two chief parts in motion, which produces the effect of striking the hours and the quarters; and all the other pieces are designed only as collateral support to the two principal ones. 3. From these observations the simplicity of construction in the new repeater is evident; and of course a great diminution of useless expense to the manufacturer."

Observations.—The original invention of those parts by which the repetition is effected, or the time struck at any particular period, we believe is due to Mr. Barlow, who applied them to a clock in 1676: but they were not applied to pocket-watches till the reign of James II, when the ingenious inventor endeavoured to obtain a patent for a watch of this kind; but was prevented by a Mr. Quare, who exhibited one of his own invention before the King and Council at the same time, and obtained the patent instead of Mr. Barlow. We have already given an account, in our fourth volume, of the progressive improvement of timekeepers;

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and described a very simple repeater which was laid before the Society of Arts, by Mr. Elliot of Clerkenwell, at p. 65, vol. i. In the present instance, the spirit of true invention, that of simplification, we think, is carried to its utmost extent: at least we cannot conceive how the same purpose can be effected by fewer parts than an *insulated hammer*; and this simplicity of construction induces us to regard Mr. Berrollas's repeater as superior to any other which we have had an opportunity of examining.

Mr. FAIRLES's *Patent for a Windlass, Windlass Bits, and metallic Hawse-hole Chamber. Dated Nov.* 1808.—*Rep. of Arts, No.* 85, *Second Series.*

THIS patentee has thought it necessary to illustrate his invention by thirteen figures, which accompany his specification, and represent the windlass, its bits, wheels, surge-bones, pinions, pall-covers, pall-box, hawse-bones, rollers, cranks, &c. From this it is evident, that its nature is such as not to admit of a perspicuous description without either figures or a much greater space than can be allotted to it in a work of this nature; hence we must refer to the Repertory of Arts for its particular construction. With respect to its action we are informed, that the surface of the surge-bones is so inclined, "that when a rope is applied round the ends of the windlass, to raise a weight, &c. the rope slips down, or is forced by the adjoining part of the said rope into its original situation, and is therefore prevented from (what seamen call) riding, that is, the one part crossing the other, which always produces much delay and inconvenience." The patentee states his invention not only to consist in making this improved windlass, but in applying any of its parts to others that are either now in use, or may be used during the term of his patent.

Observations.—How far this improved windlass may be capable of saving much time and labour, the patentee has not offered us a ready means of judging, as his description relates solely to the numerous figures representing its several detached parts, without any thing more respecting its general action as a whole than we have given. The slipping of the rope, however, is liable to the same inconvenience as in Captain Hamilton's capstan, a descriptive account of which was published in the Repertory of Arts, vol. ii. Second Series; and has been noticed at page 124, vol. ii. and page 12 of the present volume of this work. This defect, we conceive, might be remedied by the application of the principle of Mr. J. Boswell's improved capstan, described at page 11 of our last number.

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Mr. SEWARD's *Patent for a new or improved Hook for bearing up the Heads of Horses in drawing Carriages. Dated March* 1809.—*Rep. of Arts, No.* 85, *Second Series.*

THIS invention consists in fixing a spring to what is generally called the watering-hook of a saddle, for the purpose of communicating a certain freedom of motion to the horse's head, similar to that which is given by the hand of a rider. Mr. Seward's mode of effecting this is by fixing one end of the watering-hook to the centre of a circular brass plate firmly screwed to the pommel of the saddle, so as to permit it to turn round a centre. On this plate rests a circular spring, one end of which is fixed to a stud in the plate, and the other to the shank of the hook; therefore, when any force is applied to the hook by means of the bearing rein, the spring has a tendency to restore it to its place again.

Observations.—We do not perceive any thing in the construction of this hook, or rather in the application of a spring to it, which is likely to prevent it from answering the purpose for which it was intended.

Mr. JONES's *Patent for Compositions for making Trays, Waiters, and various other Articles. Dated March* 1805.—*Rep. of Arts, No.* 83, *Second Series.*

THE composition which Mr. Jones uses for making small articles consists of rope and rags, in proportion of five parts of the former to one of the latter; and for large articles, the proportion is about ten parts of the first to one of the last. These ingredients are reduced to a pulp by any of the usual methods employed for that purpose, and as much vitriol added to it as will make the whole a weak acid; or it may be used without any acid, but the patentee conceives it will not do so well without it as with it. Compositions may be made of each of these materials alone, or in various other proportions and ways; one of which is by mixing an equal quantity of dried nettles and rope, but the first Mr. J. deems the best.

To collect the pulp for the purpose of making any article, a wire sieve and frame, of a proper size and shape, according to the article to be made, is preferred. When a sufficient quantity of the composition or pulp is collected to form the article at once, it is turned out of the sieve, placed between flannel or woollen cloths and boards, and pressed lightly, in order to cause it to part with some of the liquid it contains, and reduce it to a kind of felt. It is then put into proper dies or tools, and pressed until it

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becomes solid, and assumes the shape required. In this state the cloths are taken off, and it is put into a stove or oven of a moderate heat, where it remains till it be nearly dry, when it is again put into the dies and pressed, to set it and render it smooth. The next operation is, to return the article to the oven until it be perfectly dry; and the warping is prevented by using a block to fit the article, or by any other convenient means. The articles are then smoothed by hammering, or any other method, and dipped in the liquid usually employed for japanning paper goods, and put again into the oven to dry previously to their being finished. The dies may be made of metal, or any other suitable material, and, as applied to this purpose, are regarded as a new invention by the patentee. Those which he makes use of together are one of them composed of cast iron, and the other of tin or some other soft metal. His mode of applying a sufficient degree of pressure is by means of a stamp or press, but any other means which will answer the purpose may be employed; and the use of these compositions is intended to extend to various articles besides those enumerated in the title of this specification.

Observations.—Mr. Jones's mode of forming articles of this nature will probably render them equal, if not superior, to similar articles formed from the same materials in any other manner. Compositions of this kind may be applied to several other purposes besides those enumerated in the title of this specification: among these, we think they would answer very well for small picture-frames, and which might be afforded at a much less price than they are now sold. The absorbent quality of pasteboard has caused it to be used by some of our most eminent painters; and we apprehend that excellent articles for this purpose might be manufactured by Mr. Jones's method, as they would be firmer, and not bend or warp so easily as common pasteboard, nor would they be so liable to split or to be otherwise injured by damp. If this be the case, his invention will be of considerable service to the fine arts.

Mr. Edward Stracey's Patent for an improved Method of hanging the Bodies, and of constructing the Perches, of Four-wheel Carriages, by which such Carriages are rendered less liable to be overturned; and of constructing Perch-bolts and Collar-braces. Dated January 1809.—Rep. of Arts, No. 83, Second Series.

The specification of this patent describes the construction of, and claims a patent-right for, the invention of four distinct objects: viz. 1. For a mode of constructing the perch of a four-wheel carriage, so that each of the axletrees may have a vertical

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motion independent of the other; or that they may be in different planes at the same time.—2. For a method of hanging the body of a carriage, which renders it more difficult to be overturned, and adds to the ease of its motion.—3. For the manner of making a collar-brace, "which shall almost immediately bring the body to an equilibrium, should the centre of gravity be moved."—And, 4. For a mode of forming the perch-bolt, by which the carriage is permitted to turn more freely, and the usual friction of this operation is very much diminished.

1. *Construction of the perch.*—The perch is so constructed as to admit of a revolving motion, by passing through two cylindrical boxes of metal; one of these boxes is fixed at the junction of the perch wings, and the other in the hind axletree bed. The axes of these boxes are in the same right line; and the revolving motion of the perch allows one of the axletrees to have any degree of obliquity to the horizon without affecting the other.

2. *Method of hanging the body.*—The chief variation under this head is in the body loops, which are so extended that they terminate nearly under the shackles of their respective springs. The end of each loop is formed into a cylindrical axis, which is inserted in a box or socket, and admits of a circular motion, which, in conjunction with that of the perch, the patentee conceives will add greatly to both the ease and security of the carriage.

3. *Manner of forming the collar-brace.*—These braces are fixed to the bottom of the carriage, one on each side, and pass in contrary directions round a roller attached to the upper side of the perch. Their ends being fastened to this roller, they check the vertical motion of the carriage, but admit it to a certain degree laterally, and, by acting against each other, tend to restore the equilibrium.

4. *Formation of the perch-bolt.*—This bolt is formed into two screws tapped in contrary directions, and separated from each other by a narrow collar of iron. A nut is fixed into the transom-bed corresponding to one end of the bolt, and another into the fore axletree-bed adapted to the other end. When the bolt is screwed into its place, and the fore axletree brought parallel to the hind one, any deviation from this position gradually separates the transom-bed from the fore axletree-plate, and removes part of the friction occasioned by the motion.

Observations.—Various contrivances have been adopted for the purpose of admitting the axletrees of carriages to move in vertical planes, independent of each other. Some of the original mail-coaches, made according to Mr. Vidler's patent, had perches which allowed this motion. Mr. Cooke also took out a patent for a similar invention in 1806. See our second volume, p. 523. Other patents have been obtained for superseding the necessity of perches altogether by Mr. Elliot and Mr. Houlditch; the former

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in 1806, and the latter in 1807. See vol. iii. p. 38 and 39, of this work. Whether the inefficacy of Mr. Vidler's method of construction caused it to be given up in the mail-coaches, or the goodness of the roads rendered it unnecessary, we are not prepared to say, but we are inclined to think the latter. Mr. Elliot's method we conceive to be the best which has yet been invented for effecting the purposes described in this specification; as we conceive it to be free from some objections which might be urged against Mr. Stracey's method.

Mr. John Brierley's *Patent for a new Mode of setting blue Lead for corroding the same into white Lead. Dated January* 1809. —*Repert. of Arts, No.* 85, *Second Series.*

In Mr. Brierley's process the lead is set up in stacks, which may be composed of seven or any other number of beds of dung or bark. First a layer of dung or bark is laid, and small open pots containing the acid are inserted in rows upon it; over these is laid a flooring of perforated boards to support the rolls of *blue* lead. Planks laid on edge around these rolls, and across them if necessary, support another flooring, upon which a second bed of dung or bark is put, and so on to the required height.

By this means the blue lead is confined, not in the pots themselves, from which in the common mode it is extracted with difficulty, but in a kind of chamber filled with the vapour of the acid, and when the stack is taken down, the rolls are easily carried off without any dust flying about, especially if a little water is sprinkled over them.

The several chambers may be made to communicate with each other by a flue being run through the upper beds; and thus the temperature of the beds may be ascertained by letting down a thermometer.

To prevent the upper beds of dung or bark from draining into the lower pots, the joinings of the flooring upon which they are laid are secured by thin slips of wood being laid over them.

Not only pots of less value are required in this method, but the breakage is considerably less; and much more lead can be set up in these beds than in old ones of the same size.

Observations.—We consider this method of making white lead as a very considerable improvement in the art; and recommend its adoption to the manufacture of verdigris, in which it would no doubt be attended with equally beneficial consequences.

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Mr. MASSEY*'s Patent for an improved Cock for drawing off Liquors. Dated September* 1808.—*Repertory of Arts, No.* 83, *Second Series.*

WHEN we inform our readers that the specification of this patent cock is accompanied by fifteen separate figures, we apprehend that they will excuse us for not entering into any minute description of it without the assistance of the figures, but referring to the above place for the particulars of the apparatus. We shall, therefore, only observe, generally, that this cock consists of three separate valves, which may be used either together, or singly, as the maker or purchaser may think proper; each of these is so constructed that the liquor may have a tendency to close it. It is also attended with a suitable apparatus for fixing it to the barrel. And both are constituted of a combination of stuffing boxes, levers, springs, collars, rods, staples, hooks, bars, plates, studs, screws, and cranks. By a particular contrivance, the use of the key is rendered as necessary to take the cock out of the barrel as to unlock it; and that all possible security may be given to the contents of the cask, the key is prevented from coming out of the cock except when it is locked.

Observations.—The complicated nature of this apparatus will render the first expense of it considerably greater than that of others of a common construction; at the same time that the number of its parts will cause it to be more frequently out of order: and both will certainly operate against its general adoption.

Mr. STEERS*'s Patent for a new Method, directed by Machinery, of using the Screw, by which its mechanical Power, or its Motion, is increased. Dated March* 1809.—*Repertory of Arts, No.* 84, *Second Series.*

THIS new method of applying the mechanical power of the screw may be accomplished by the three following modes:—1. By the screw and nut being made to revolve together, either in the same or in contrary directions. When they are made to revolve the same way, and the one moves faster than the other, an increase of power is obtained; but when they revolve in contrary directions, an increase of motion is the effect. 2. By two screws placed opposite to each other, and turned together, either in the same, or contrary directions, their nuts being fixed. 3. By the nuts revolving together, the screws being fixed. These effects

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may be obtained by various modes and combinations of machinery: the following is that described by the patentee.

Two wheels, one of which is fixed to the nut and the other to the screws, have equal diameters, and are turned by the same pinion. One of these has one tooth more than the other; and supposing one of the nuts to have 101 teeth, and the other 100; when the former has made one revolution, the latter will have accomplished one, and the hundredth part of another; and therefore, the screw will have risen one hundredth part of the distance between two of its threads, and the increase of power obtained will be in the ratio of 100 to 1. Again, if another nut and screw revolve round a pin in the centre of the upper part of the former screw; and if this nut and screw support two wheels with 102 and 101 teeth respectively; when these are turned by the same pinion, the nut will descend through a space equal to the one hundredth and first part of the distance between the threads of the screw for each revolution of the nut. The turning of the two first-mentioned wheels then has the effect of raising the nut the one hundredth part, and the turning of the two last-mentioned wheels, the effect of lowering the nut the one hundredth and first part of the distance between the threads of the screw; consequently, when the wheels are turned all together once round by the same pinion, the effect will be that of raising the upper end of the screw the ten thousand one hundredth part of the distance between two of its threads, and the increase of power obtained by this mode of application will be in the ratio of ten thousand one hundred to one. When an increase of velocity in the screw is the object to be attained, an apparatus is applied which causes the two first-mentioned wheels to turn in contrary directions; the screw and nut then move in opposition to each other, and the end of the former rises with an increased velocity.

When the two screws, with their nuts, are placed opposite to each other, and put in motion by machinery as above, if they move in the same circular direction, one screw will advance and the other recede; and the same with the nuts if they be put in motion: but as the motion of one of them will be quicker than that of the other, the distance between them will be gradually diminished, and an increase of power obtained, as before.

Observations.—Mr. William Hunter published a method of increasing the power of the screw in the Philosophical Transactions for 1781. This consisted in making a double screw, with the threads of one end finer than those of the other, work in two nuts, one of which was fixed and the other moveable. The nuts were caused to approach each other by the screw being more raised in the fixed nut by means of the coarser thread than the moveable nut was lowered on the screw; and the power was increased proportionally to the difference of these motions. Mr.

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Steers's method considerably extends this principle of Mr. Hunter with respect to power; but the latter gentleman's is more simple, and better adapted to measure minute spaces.

Mr. Peck obtained a premium from the Society of Arts for a screw-machine for gaining power. This was effected by causing nuts to revolve round fixed vertical screws. The nuts had pinions attached to them, and were turned by endless screws, fixed horizontally, to a platform which could be easily raised or lowered at pleasure. This contrivance was inferior in power to Mr. Hunter's; but had the advantage of forming a double press by working both upwards and downwards alternately; thus, while a portion of materials, which had been sufficiently pressed, were relieved for the purpose of being taken out, another portion received the action of the press, by which means a considerable saving of time was obtained, which would, perhaps, be greater than that effected by an increase of motion in the present instance: but where a great pressure is the sole object, it will be better accomplished by Mr. Steers's invention than by any other method with which we are acquainted.

AGRICULTURE.

On Moss Husbandry. By the Rev. Mr. HEADRICK, *of Dunichen, near Forfar.—Farmer's Mag. No.* 38.

THIS communication contains remarks on the conversion of moss into manure, and the conversion of it into soil. The writer states, that the method of mixing moss with dung has been partially practised in Scotland from time immemorial, and he thinks that if the foul water which runs from great towns were collected in reservoirs, and made to act upon moss, a prodigious quantity of valuable manure might be procured. An instance is detailed of this practice on a small scale by Mr. Reid, of Ratho, who collects all the foul water which issues from his premises into a reservoir, where it is either mixed with moss, scourings of ditches, &c. or is carried out and sprinkled upon the land from a large cask fixed on a cart in a similar way to that by which the roads are watered in the vicinity of London; and the result has been, that he has by these means brought into cultivation a considerable extent of waste land from his own resources, instead of bringing dung from Edinburgh at a vast expense. The chief point, however, to which the attention of the reader is directed, is a method of converting moss into manure without any mixture of lime or dung, or any extraneous substance; and this method is given according to the practice of Mr. Mylne on his farm near Paisley. The field which was the subject of the experiment contained about twenty acres, and was a most sterile and intractable soil; parts of it had

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been limed to excess, and then cropped with oats till they no longer returned the seed; parts of it carried rushes and stunted heath, and some portions of gravelly soil produced stunted broom; and much labour had been employed to render it dry, by making open and covered drains: at the south end of the field about an acre was covered with moss. In order to reduce this moss to cultivation, a very large slope drain was made along its boundary with the solid land, and with the moss thrown out of this drain experiments commenced. It was laid in a long ridge, and part of it mixed with dung in various proportions, part of it with lime, and a large portion remained without any mixture whatever. As it was feared that the lime might render the moss too dry, and by that means counteract its decomposition, water was repeatedly thrown upon the heap, and poles thrust down here and there to open a communication with the interior parts, and the whole was turned more than once, water being sprinkled on it during the operation. A field cleanly fallowed was chosen for the application, and part of it manured with dung from Paisley, at an expense of four pounds per acre, the remainder being dressed with the three different kinds of compost from the drain, and the best judges could not perceive any difference in the appearance of the crops corresponding to the different kinds of manure; and what was least expected was, "that the crop which had got only chopped and watered moss, without either dung or lime, was fully as good as any of the rest."

It is stated to have been found a great improvement in the conversion of moss into manure without addition, to lay out the moss at the depth of a spade on some dry contiguous ground, since, by exposure to the air and moisture from the heavens, it soon becomes brittle, and moulders down into a friable substance; and the process may be hastened by the occasional use of the plough, to cut and mangle it in various directions: when it begins to produce chickweed in abundance it is judged fit for use, and may be advantageously mixed with lime or dung, and will also be found excellent manure without the admixture of either. Mr. Stirling, of Carsbank, is said to have adopted this method with success, on what is called *till* or cold-bottomed land.

The kind of moss which has been the subject of these experiments is termed by Linné *sphagnum palustre*; but as mosses vary as much as soils, it is recommended to make similar experiments with those of different species.

With respect to the *conversion of moss into soil*, a reference is made to an essay in a volume of the Communications to the Board of Agriculture, published before the commencement of our labours; but the present paper is confined to the practice of Mr. Pitlow, at his farm of Deans, near Bathgate, who has succeeded in converting into very fertile soil a considerable portion of moss, without the application of lime or dung, or any manure whatever,

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though his moss land appeared to be one of the worst that could be selected for the purpose, since its surface was occupied with coarse grass and heath, and various species of lichens, as well as the *Sphagnum palustre*. The first operation was to dig a long deep drain, to discharge the water from the outlet of this moss; drains were then made between the moss and the solid land on each side, as well as through the centre, and the whole was afterwards intersected so as to divide it into square portions of eight or ten acres each. Various methods were adopted to render it fit for cultivation, such as paring and burning, ridging up with the spade, &c. but without effect: the plan at last adopted was to trench the moss to the depth of from two to three feet, by which means the porous woolly stuff, with the heath and coarse herbage, were thrown to the bottom, while a more dense and compact species of peat was obtained to form a soil upon the surface; this trenching was executed, in regular spits or courses, from one main drain to another, and the bottom of each shovelled and smoothed, to prevent the stagnation of water, and the turf was regularly arranged with the tops downwards. The surface thus obtained soon acquired fertility from the action of air and moisture, and the disorganization was hastened by frost; and as soon as it had acquired sufficient tenacity to bear the tread of cattle, a light plough was employed to change the exposed surface. As soon as chickweed appeared, grass-seeds were sown without any corn crop; and as no ridges or water furrows are seen on the surface, the concealed drains serving the purpose of the latter, the whole exhibits the appearance of a smoothed lawn. The success of this experiment induced Mr. Pitlow to proceed in reclaiming moss lands after the same manner for corn crops as well as for grass, and the results are stated to have been equally satisfactory.

Observations.—The plan of using moss as a manure, without any mixture of other dung or of lime, but by the simple preparation of turning it for the sake of exposure to the atmosphere, is neither a new discovery, nor is the practice confined to the northern part of Britain; but as it is not universally known, the promulgation of it through the Farmer's Magazine may be productive of public utility.

The mode of reclaiming moss land, by inverting the sods after cutting them up at two or three feet in depth, is (we believe) now made public for the first time, and merits the consideration of those who have such lands to bring into cultivation. It appears to us, that it would be more judicious to divide the land to be reclaimed into oblongs instead of squares, as by this measure every portion would be nearer a drain than the centre part of a square; the size of the divisions must be immaterial, but they should rather be too small than too large.

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On the Inutility of smearing Sheep. By A. S.—*Farmer's Mag. No.* 38.

THIS writer states, that he possesses a sheep-farm in a very high and cold tract, and has made many experiments on salving or smearing sheep with various kinds of unguents, and that he has satisfied himself that the practice of smearing is useless even in very high climates. In order to prove this, he purchased two score of Cheviot sheep, which had been smeared in former years, and these he did not smear, notwithstanding the clamour of his neighbours, who predicted as a consequence the ruin of their flocks, as well as his own, from the scab; the wethers, however, were every one alive the succeeding summer and in good health, and their wool in most excellent order, although a more severe winter had been seldom experienced. But, instead of smearing with any kind of unguent, he rubbed them over with tobacco-water, which was easily done and at a small expense, and which effectually destroys all vermin that affect sheep. All the usual arguments in favour of the practice are then combated with success, and the decision of Mr. Luccock is quoted from his Essay on Wool in support of this opinion, who holds that "when wool is affected by the filthy custom of smearing, the pile is less capable of acquiring the softer and more delicate tints, which it is often desirable to communicate to the different articles of the woollen manufacture; and that in the subsequent processes this filthy staple produces much greater inconvenience, and is subject to more considerable waste, than the purer pile."

A subsequent paper by the same writer expresses his conviction of the truth of these positions from two years more experience, and asserts, that dressing with tobacco liquor is more effectual for destroying ticks than dressing with oil, as well as being cheaper, the latter costing three-pence for each sheep, while the former may be applied at an expense of only four-pence for a score.

Observations.—These experiments evidently shew, that the disgusting custom of smearing the flocks which are fed on the high mountains of the north, is by no means necessary to the health of the animal; and when Mr. Luccock's accuracy and experience as a manufacturer is allowed its due weight, his opinion as to the effect upon the wool may be regarded as decisive.

On making Butter. By M. F.—*Farmer's Mag. No.* 38.

AFTER noticing and censuring a mode of making butter which was given two years since in the Magazine, and noticed in the

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third volume of this work, page 110, the writer proceeds to describe the method she pursues. It is stated, that the cows (from ten to twelve in number) are milked at eight in the morning, and at the same hour in the evening; and each cow's milk, or that of two together, is *seithed* (set) in a wooden bowl as soon as milked, and placed upon wooden shelves in the dairy, where the evening's milk remains 34 hours, and the morning's 22 hours, when the cream is skimmed off, and the milk afterwards made into cheese, which sells from six to seven shillings per stone. The butter is churned once in the week, and the churn is filled with boiling water, and worked for a short time on the day preceding the churning. When the butter is taken out of the churn it is worked till perfectly free from milk in a clean vessel among spring-water, and is then weighed, and for every stone ten ounces of salt are taken and mixed with it, though in May and June one ounce more of salt is required, and after the month of August, one ounce less than this quantity; the salting process is carefully performed with the hand, as the perfect incorporation of it with the butter is held to be essential, for the preservation of the latter. When thus salted, the butter is put down into a clean kit, and covered over with a handful of salt, till the produce of the succeeding week is added to by being closely pressed down, and covered with salt as before. Salt-petre is never used by this writer.

Observations.—As this method of preserving butter challenges a comparison with that which appeared in a former Magazine, we should shrink from our duty if we did not give the preference to the mode described in this communication; for the quantity of salt here used, is sufficient to keep the butter sweet for a year, within which time it is generally used, and at the same time does not give it that very pungent and unpleasant taste which is too often found in the butter imported from Ireland.

A new Plan for sowing Turnips.—Farmer's Mag. No. 38.

The method described in this communication having been found to answer on a small scale, it is communicated to the public, because thought deserving of a further trial. The writer took a pound of turnip-seed, and mixed it among two pounds of common tar, stirring the mass till all the seed appeared regularly mixed with it; the mixture was then put into a watering pan, all the holes being covered, except one made large enough to allow a seed covered with tar, to run out; the seed was in this manner dropped into a drill and covered with a light roller: the day being warm, the tar ran freely, and the seed came up more regularly than if sown by any machine whatever. The peculiar advantages

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are supposed to be, that turnip-seed anointed with tar, will not be apt to receive damage from either hot or wet weather.

Observations.—It is not improbable, that anointing turnip-seed with tar, previous to committing it to the ground, may obviate many of the difficulties of raising a crop of turnips in dry weather; and if so, the discovery is a very important one; it cannot, however, be generally recommended till repeated experiments have established its utility.

Description of a new Mode of yoking Horses in a Threshing Machine. By M. H.—Farmer's Mag. No. 38.

This method (which cannot be so well explained without a reference to the plate which accompanies the description) consists in making the shafts by which the horses draw in a curved form, to coincide with the path in which the horses walk; and by using traces like those of a stage-coach affixed to the radius or beam which projects from the upright shaft of the horse-wheel, the inner trace being somewhat shorter than the outer one; and these traces are so managed by being bent round two pulley-wheels, that the two horses supposed to be employed are affixed to the same trace on the *near* side, and to the same trace on the *off* side; and this is stated to produce a more regular and equable motion of the machinery, and also that each horse has his own share of the draught, as much as when yoked abreast with swing-trees.

Observations.—It is certainly desirable, when two horses are employed as the moving power of machinery, that each should take his proper share of the draught, and in the proposed method of yoking them that will be the case while they both continue in motion; but if one stops without the other from any cause, a derangement of the whole system is produced, because the traces are not fastened to the machine, but only rolled round a pulley, that the power of one horse may counteract the power of the other; so that when one power is taken away, the remaining power is without control. Though this reasoning may seem very confused, and be almost unintelligible, yet it will appear very clear and satisfactory, from an examination of the plate in the original publication.

Notices as to Lucern. By A. S.—Farmer's Mag. No. 38.

It is stated, that in a garden at Laurieston near Edinburgh, a plat was sown with lucern and red clover seeds, in alternate drills, in the beginning of May 1807; and as there was no corn crop, the plants of both flowered and were cut down that season. On the

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23d of May in the succeeding spring, the lucern was fit for cutting, being eighteen inches high, while the plants of red clover were not more than six inches. On the 16th of April 1809, the young shoots of lucern were sixteen inches, and those of the clover not exceeding one inch. The clover and the lucern were both cut three times in the course of the summer of 1808, and the clover was thought to have been the larger crop on the whole, though the lucern was the highest and the earliest ready; and the writer considers that lucern has this peculiar advantage, that it will remain in the ground, and even increase in bulk of crop, for the space of ten years at least, while, on the other hand, red clover lasts in the soil only two or three years. And lucern is asserted to be well worth trying by the farmer in any small paddock of deep loam, sand, or gravel, near his house, for soiling horses, cattle, and hogs, or as an early feed for ewes and lambs.

Observations.—This very useful plant is much less generally known than its merits deserve, and every attempt to diffuse a knowledge of its usefulness is entitled to approbation. Perhaps we cannot recommend it in stronger terms than by assuring our readers, that lucern has never been abandoned in any district, into which it has been yet introduced, but has been more and more extensively cultivated.

The Merino Cause—Description of His Majesty's Spanish Sheep,—and final Reply to the Dishley Quack. By B. THOMPSON, *of Red Hill, Notts.—Agricultural Mag. No.* 21.

The Coalition, or united Evidence of Bakewell, Thompson, and Cultivator, in Defence of the Merino Cause. By JOHN HUNT, *of Loughborough.—Agricultural Mag. No.* 22.

On the Liberality of public Discussion, and the Illiberality of personal Abuse; in Answer to Mr. Thompson. By JOHN HUNT, *of Loughborough.—Agricultural Mag. No.* 23.

THESE papers are a continuation of the controversy noticed in the last number, page 76, and neither pleasure nor information will be collected from the perusal. The controversy is, we learn, now deciding in a more satisfactory manner than by the pen, upon the farm of Mr. Wright, of Pickworth, near Stamford. Six of each sort of the same age and sex are chosen for the experiment, and two acres of seeds, in every respect similar, are fenced off for each species: they were all weighed in alive, separately, and will be weighed out the same, and the fleeces of each be weighed and sold.

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On the Encouragement due to our home-grown Wool, and on the Weather. By a GRAZIER.—*Agricultural Mag. No.* 21.

THE writer, lamenting the excessive price of fine cloth, apprehends, that if we were, one and all, to wear no woollen cloth but what is manufactured from the fleeces of our own sheep, it would have a great effect upon our trade, and tend to lower the exorbitant prices exacted for fine cloths, and in the end prove highly beneficial to the country at large. He wishes it to be considered as no degradation of the patriotic landlord, to wear a coat manufactured out of English wool, and to clothe his family and servants in the same, and set the example to his tenantry of being satisfied, for clothing, with the produce of his own country.

The observations on the weather are extremely unimportant, and rather inquire after than communicate information.

Observations.—The recommendation of the Grazier, for the higher classes in society to wear no other woollen cloth but what is the produce of our own country, would be productive of national advantage in every point of view, if it was attended to; for it would operate as the most powerful of all inducements to improve generally the quality of the wool, and would render our manufacturers independent of a foreign supply for the exercise of their industry. The example would soon be followed by the middle classes, and the effect of fashion would bring about a result which could scarcely be hoped from legislative interference. The custom which has for some years prevailed at Lincoln, of no ladies being admitted to the summer county balls, but those who are dressed in the stuff-manufacture of the county, has done more to improve the woollen manufacture, than all the bounties and premiums which have ever been suggested.

On Smut in Wheat, in Answer to T. B., and on the supposed Predominance of Pasture Land. By CULTIVATOR MIDDLESEXIENSIS.—*Agricultural Mag. No.* 21.

On the Diseases of Wheat, in Answer to Cultivator Middlesexiensis. By T. B. *of Lidlington, Bedfordshire.—Agricultural Magazine, No.* 22.

On the Diseases of Corn, in Answer to T. B. By CULTIVATOR MIDDLESEXIENSIS.—*Agricultural Mag No.* 23.

On the Diseases of Wheat, &c. By T. S. S.—*Farmer's Magazine, No.* 38.

As all these communications are merely discussions, without leading to any practical conclusion, they are noticed only for the

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purpose of directing such of our readers to them as have the curiosity of knowing what may be said upon the subject. The three papers in the Agricultural Magazine are purely controversial; that in the Farmer's Magazine is not devoid of sensible and acute observation.

Query on the best Mode of sowing artificial Grasses. By G. M.—*Agricultural Mag. No. 21.*

On the best Method of raising artificial Grasses. By TYRO.—*Agricultural Mag. No. 22.*

As these two papers are so intimately connected with each other, it was thought unnecessary to consider them separately. The author of the Query is informed, that the general method of cultivating artificial grasses is to sow the corn crop broad-cast, and, having reduced the earth to a very fine tilth, to sow the seeds, harrowing them once over with some light harrows; but Tyro apprehends the culture in rows to be highly favourable to producing good crops of clover; for a crop of Lammas wheat was drilled by a horse-hoe in rows eight inches asunder, and the following April clover was sown upon it broad-cast, and harrowed in, and became a capital crop: preference is however given to a crop of spring wheat, or barley, or oats, as a covering crop to clover over Lammas wheat, as then the field will have been more recently stirred; and it is recommended, if it can be accomplished without much inconvenience, to drill the spring corn: the quantity of seed usually sown by this correspondent is twelve pounds of red clover, four pounds of white, two pounds of trefoil, and four gallons of ray-grass to the acre.

Observations.—The practical information contained in this short paper is founded on right principles, and supported by experience; and the recommendation may be followed without incurring any risk of ultimate success, especially if noxious weeds are carefully destroyed, while the clover plant is young.

Query on Devonshire Cattle. By PHILORURIS.—*Agricultural Magazine, No. 22.*

THIS gentleman possesses a farm in Kent, which he took into his own occupation at Michaelmas last, and his predilection for Devonshire cattle inclined him to purchase his cows of that breed; for he had uniformly found in agricultural publications a high character of the Devon cattle, not only for the excellence of their beef, but also for the richness of their milk. It having, however, been represented to him that this breed of cattle is the

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most unquiet and mischievous of any, and most destructive to fences, by destroying them with their horns, and, moreover, that the young cows give so little milk in the winter season, as not to repay their keep; he has suspended his intention till he receives farther information through the Magazine.

Observations.—We forbear giving an answer to this query, because we might thereby take the subject out of the hands of the correspondents to the Magazine in which it has appeared; but we wish *Philoruris* had endeavoured to conquer the doubt by trying the experiment, instead of seeking his information through the medium of the press; a mode evidently uncertain as to lapse of time necessary for procuring it, and at last less satisfactory than a personal experiment.

Experiment on Paring and Burning. By G. S. *of Hants.*—*Agricultural Magazine*, No. 22.

The field which was the subject of this experiment, was ten acres of strong cold clayey loam, which had lain as a kind of waste, perhaps for centuries. The operation commenced in the middle of April; and such part of the surface as was level, was turned up with the common plough; and the clods thrown in heaps; and the grubbing up of bushes, levelling holes, and removing stones, was done at a very heavy expense. These impediments prevented the completion of the ploughing till the end of July, when fire was applied to the heaps, which reduced them to ashes very speedily, and without leaving any unburnt refuse: the whole expense is stated to be about 3*l*. The quantity of ashes was very great in consequence of the depth of soil taken off in some places; they were evenly spread and ploughed in in seven-bout lands; and some rain falling early in September, the field obtained the appearance of a most beautiful fallow, and the soil turned up in all the best parts in rich crumbling mould. The field was then sown with wheat in the proportion of two bushels to the acre, which came up very quickly, and was so strong a crop, that at Christmas it was expected to yield no produce but straw. The crop was much laid in consequence of its rankness, yet the produce exceeded four quarters upon an acre, but the grain was too much discoloured to be marketable for seed. A succeeding crop of beans was productive beyond all expectation, and another crop of wheat, the third year, is stated to have produced half a quarter per acre more than the first crop.

Observations.—The success of this experiment ought to remove all doubts respecting the ultimate advantage of improving wastes and cow commons, notwithstanding the amount of the

first expenses, which, however great, will ninety-nine times out of a hundred be most amply repaid by the future increased productiveness of the soil.

On the Threshing Machine. By R. R. *of Kelvedon, Essex.—Agricultural Magazine, No.* 22.

The object of this paper is to recommend a threshing machine which may be had of a Mr. Jones, of Clement Lane, Clare Market, London, at the price of sixty guineas. It is stated that two horses work it with ease, and that it will thresh four bushels of wheat per hour, and requires two men and two boys to work it and clear away the straw, and that it is not liable to be frequently out of repair.

On Turnips. By a Hampshire Farmer.—*Agricultural Magazine, No.* 23.

In this paper it is recommended, as a remedy against the scanty crops of turnips, which are so frequently seen, to sow the seed as early as the month of April, instead of the end of May and in June, as is generally done; and this recommendation is given on the experience of an old farmer of the writer's acquaintance, and in conformity to the practice of gardeners, and it is asserted that the danger of the plants running to seed before the winter is comparatively trifling, and that a full crop is certainly secured.

Observations.—The suggestions of this writer are deserving the notice of experimental agriculturists, and we hope the utility or inutility of the recommendation will be ascertained by fair trials.

On the intended Destruction of Wood-pigeons in Norfolk; and buying Scots Cattle by Commission. By a Countryman.—*Agricultural Magazine, No.* 23.

It is stated, that at a recent meeting of the Norfolk Agricultural Society it was proposed to offer premiums for the destruction of wood-pigeons; and it is contended that such measures should not be precipitately entered into, because these birds, in all probability, destroy a vast quantity of insects, and the seeds of weeds, which would be more destructive than the ravages which they themselves can possibly commit.

The obtaining Scotch cattle by having them purchased on commission by intelligent men is said to have been recommended

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by Mr. Coke of Holkham at the same meeting, and Colonel Cunningham to have been named by him as a gentleman possessed of adequate knowledge, and of tried integrity.

Observations.—The proposition to entirely destroy the breed of wood-pigeons in the county of Norfolk seems to have been made without due consideration; and the meeting were clearly of the same opinion, for the proposition was not adopted. And with respect to purchasing Scotch cattle through the medium of an agent, instead of buying them of the dealers who bring them to fairs, the idea appears to have been taken up rather with a view of bringing Colonel Cunningham to the notice of the meeting, than from a consideration of the result of such a practice, as interfering with the regular supply of the established fairs and markets.

On the Fiorin Grass. By M. A. *of Cobham, Surrey.—Agricult. Mag. No.* 23.

This paper owes its origin to the very laborious production on the same subject by Dr. Richardson, of the university of Dublin, which was published in the last volume of the Communications to the Board of Agriculture, and noticed in this work, vol. iv. p. 472. The writer disclaims any intention of depreciating the good qualities of this new grass, but wishes its utility for both dry and spungy soils to be ascertained by experiments in England: he inquires whether it has been tried in this country, upon what soils, and with success, what is the mode of its propagation, and where it is to be obtained.

Observations.—If this writer had attentively read Dr. Richardson's memoir, before he had sent his paper to the Magazine, he would have discovered that many of his queries were there satisfactorily answered. At the same time it is not unreasonable to doubt as to the universality of the excellence of fiorin grass, without distinction, when it is remembered how many disappointments have been experienced in the last half century from grasses introduced from Guinea, America, and the Scotch Highlands, with equally magnificent recommendations.

On Garden Radishes; and on Lettuces, as a Food for Swine. By a Clergyman.—*Agricultural Mag. No.* 23.

The writer complains that he cannot obtain any radishes in his own garden, but what are hot and fibrous as well as dwarfish and deformed, though he has tried every change of seed he has been

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able to procure; and this failure is attributed to the soil of the garden being harsh and inclined to clay, which he has endeavoured to alter, but in vain. The jet, however, of the communication is to inform the public that he has fed store-pigs with lettuces for a considerable time, and found the pigs improved full as well as on other meat: the Dutch brown seemed to him to be attended with most profit for this purpose; and though he does not decide "that it would be worth while to grow lettuces in the field for the purpose of feeding pigs, he entertains no doubt of the profit of casting to them the superabundance of the garden."

Observations.—We certainly agree in opinion with the reverend author, that it is more advantageous to give the lettuces, which are not wanted for the table, to store-pigs, than to throw them to the dunghill; and we doubt not but that every poor man, who possesses a garden and a pig, might have communicated the same *important* information to the editor of the Magazine.

Method of ascertaining the Value of growing Timber-trees at different and distant Periods of Time. By Mr. CHARLES WAISTELL, *of High Holborn.—Trans. of the Society of Arts, Vol. XXVI.*

THIS gentleman, being desirous of devising some means of estimating what might probably be the value of plantations at different future periods, was thus led to construct a table for this purpose; but this was no sooner completed than other tables seemed necessary, and he was progressively led on to the construction of the set of tables which form this paper. From information obtained from various authors, and among his acquaintance, respecting the measure of trees in girt and height at different ages, he collected that the increase in the circumference of trees is generally from about one to two inches annually, and from twelve to eighteen inches the annual increase in height; though some may fall a little short of, and some exceed, these measures.

To give an analysis of the contents of these tables, without giving the tables themselves, would only occupy the pages of the RETROSPECT, without superseding the necessity of a reference to the original publication. It appears, however, from them, that the rate per cent. of increase becomes less and less in every period of four years into which the tables are divided, and that in *sixty* years timber ceases to increase five per cent. annually. Some of the tables relate also to the thinning of timber plantations, and others to the most advantageous height to which the branches may be trimmed off, to regulate the length of the body of the tree; and though it is impossible to follow these observations and calculations in any abridged form, yet the result obtained is, that the annual increase in the bodies of trees by their growth ceases to be

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equal to five per cent. per annum, some time between forty-six and sixty years of age, according as the bodies are shorter or longer.

Observations.—This paper of Mr. Wastell contains much valuable information on the subject of timber plantations, and merits the attentive perusal of every gentleman possessed of landed property. The positions of the writer are either self-evident, or the result of experience, and the results are obtained with sufficient accuracy to be admitted as general conclusions. Nor is the paper wholly uninteresting to the statesman and the politician, as it evinces that a supply of timber for the navy, which it is generally allowed should be from eighty to one hundred and fifty years growth, according to the quality of the soil, will soon cease to be preserved for that purpose without a considerable advance in price on timber of that quality, as it will cease after sixty years growth to pay five per cent. to the owner, and few private individuals will be found possessed of sufficient patriotism to preserve their timber beyond sixty years with a loss to themselves, merely to benefit their country, unless an adequate addition of price may offer an inducement. Hence the necessity of bestowing the greatest care on the royal forests, and the utmost watchfulness to prevent their alienation, as in the lapse of another century these will probably be found the only resources from which timber can be obtained sufficiently large for building the highest class of ships of war in the royal navy of Britain.

An Account of the planting 338,199 Forest-trees at Heffleton, Dorset. By A. Bain, M. D. *of Curzon Street.—Transactions of the Society of Arts, Vol. XXVI.*

These plantations were first commenced, on a small scale, on land of the value of about five shillings an acre, and success in this way induced Dr. Bain to enclose 250 acres of heath ground with a ditch and bank, for the purpose of planting it with firs. The land was of a bad quality, and had never been valued to a tenant at more than one shilling an acre. It was very securely fenced by a bank thrown up and planted at the top with furze, and guarded with fence hurdles, and upwards of two thousand trees were allotted to each acre, and the ground was prepared for receiving them, by paring off, where each tree was to be planted, a round thin piece of turf, from one and a half to two feet diameter, with a sharp spade, which turf was thrown away, and the small spot of ground, so divested of its heath, was then dug or chopped one foot deep with a spade, or, if hard and stony, with an axe-hoe, by which the roots of the heath were completely divided; and these spaces of ground, without taking out any of

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the earth, were allowed to rot and mellow one or two years before they were filled with plants; this preparation cost about a guinea an acre. As the land intended to be planted was near the coast, and much exposed to the strong sea winds of the south coast, the planting was begun to the south-west of the whole with Scotch intermixed with spruce and pinaster, as best calculated for the soil and exposure, and were put in at three years growth; the interior was intermixed with deciduous trees. Of the whole number planted there were, Scotch fir 289,555; oak 4,362; larch 12,290; Spanish chesnut 5,647; spruce 3,450; ash 11,050; pinaster 1,900; sycamore 4,050; birch 1,700; and hazel 4,195.

The plantation is stated to have succeeded remarkably well for so poor a soil, and all the firs, except the larch, to be in a thriving healthy state, though the pinasters grow nearly as fast again as the others; and though the proportion of deciduous trees is so small, they have a very unpromising appearance, particularly the oak, chesnut, and larch; the ash and sycamore grow slowly, but afford hopes of better success when they are better sheltered; next to the Scotch firs and pinasters, the birch succeeds best in every situation.

Observations.—The experience of Dr. Bain may serve as a useful index to future planters on the southern coasts, what kind of trees to reject, and what to retain, in plantations on heathy soils. The mode adopted here of opening the ground for a year or two previous to putting in the young plant cannot well fail of being beneficial in almost all plantations on poor soils, and merits very general imitation. Ed.

HORTICULTURE.

On the Culture of Sea-kail. By Domesticus.—*Farmer's Mag.* No. 38.

The author claims insertion for his article on the plea of the near connexion between the *field* and the *garden*. He considers sea-kail as an agreeable substitute for asparagus, and sooner ripe for the table by a month or six weeks. He cultivates it by sowing the seed in the month of April in drills, on a good, dry, light soil, and by thinning the plants as they rise, and keeping them clean. In the first winter he earths them up, to protect them from the frost, and leaves them at spring eighteen inches asunder. At Christmas the decayed leaves are taken away, and each of the plants is covered with a large deep pan or flower-pot, upon which is laid a quantity of the leaves of trees, to keep off the frost, and produce heat to the plant. In April the pots will be

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found filled with fine tender-branched shoots, which may be cut over by the ground, and the stumps covered up again for a second crop. The sea-kail with this treatment, if sufficiently boiled in two waters, is asserted to be equal to any asparagus or brocoli, and may be eaten with any sauce best adapted to the taste.

Observations.—The trouble attending the culture of this vegetable will always prevent its general introduction into gardens; but where the necessary time and labour can be spared, it is well worthy the attention of the horticulturist, and the method here pointed out for its cultivation may be safely relied on for ensuring success.

CHEMISTRY AND MINERALOGY.

An Account of some Experiments, performed with a View to ascertain the most advantageous Method of constructing a Voltaic Apparatus, for the Purposes of chemical Research. By JOHN GEORGE CHILDREN, *Esq. F. R. S.—Phil. Trans. Part I. 1809.*

MR. Children's battery consisted of 20 plates of copper and as many of zinc, each four feet high and two feet wide, connected together by slips of lead; their whole superficies amounted to 92,160 square inches. The wooden trough was rendered water-tight by cement. 120 gallons, consisting of three parts of fuming nitrous acid, one of sulphuric acid, and thirty of water, was the charge of the battery.

With the assistance of Messrs. Davy, Allen, and Pepys, the following experiments were made:

1. Eighteen inches of platina wire, of $\frac{1}{30}$ of an inch diameter, were fused in about twenty seconds.
2. Three feet of the same wire were made red hot.
3. Four feet of the same wire were rendered very hot, but not red.
4. Charcoal burnt with great brilliancy.
5. Of iron wire only ten inches were fused, and three feet ignited.
6. No effect was produced on imperfect conductors.
7. The electrometer was not affected.
8. The shock hardly sensible on the moistened cuticle, not at all on the dry.

Comparative experiments with a battery similar to the couronne des tasses of Volta, and composed of 200 pairs of plates of about two inches square, placed in half-pint pots of common queen's ware, charged with the old acid, strengthened with the addition of about a quarter of a pint of sulphuric acid to each gallon:

1. It readily decomposed potash and barytes.

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2. It quickly metallized ammonia.

3. It produced the vivid ignition of charcoal.

4. It caused a considerable divergence of the gold leaves of the electrometer.

5. At the end of three hours it gave a vivid spark; at the end of 24 it still metallized ammonia, and continued to do it, with a gradual decrease of energy, for 41 hours, when it was nearly exhausted.

These experiments confirm Mr. Davy's theory, that "the intensity increases with the number, and the quantity with the extent of the series."

It is strikingly confirmed in the first and fifth experiments with the large battery, in the subsequent ones on imperfect conductors, and in those with the small battery. Platina wire, as a perfect conductor not liable to be oxydated, affords free passage to the electricities, which, by their mutual annihilation, evolve sufficient heat to fuse the platina.

The low intensity of the electricities (denoted by the electrometer) explains the different effect with the iron wire, which on the completion of the circuit acquires a crust of oxide, and becomes a bad conductor; to this cause too must be assigned the weak action of this battery on imperfect conductors: on the contrary, the combination of small plates, on account of its intensity, effects with ease the decomposition of imperfect conducting substances. In the construction of the small apparatus, it is requisite, on many accounts, to have the cells of a large capacity; and in an extensive combination there is an absolute necessity for a certain space between the plates, to prevent spontaneous discharges, which were witnessed by Mr. C. in a battery of 1250 four-inch plates of the new construction. The author here makes a digression to introduce an experiment, in which he ascertained the striking distance of the Voltaic spark. This fact, he says, must silence the objection against the identity of common and Voltaic electricity founded on the contrary opinion. The striking distance was $\frac{1}{50}$ of an inch. To those who compare this short distance with the length of the spark of the common machine Mr. C. answers, that to increase the striking distance, it is only necessary to increase the number of the plates; and as the intensity is proportionable to the number, it may be so extended as to pass through a greater space than the common spark. Another proof of the identity of the two electricities appears in the similarity of their lights in vacuo. The battery of small plates acted with great energy on imperfect conducting substances, but it only fused one half inch of the platina wire used in the first experiment, though, had the effect followed the ratio of surfaces, it should have fused nearly 14.

The effect then of the battery seems to be as the compound ratio of the number and size of the plates, one the cause of intensity,

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the other of quantity. In the construction, therefore, of an apparatus, its intended use must be considered. For experiments on perfect conductors large plates are best adapted; on imperfect conductors the number must be increased and the size may be diminished; and for general use, when both intensity and quantity are required, plates of four inches square will be found most convenient.

The old method of soldering the plates of copper and zinc together, and fixing them in the trough by cement, is much inferior to the new one, which joins them only in one point, allows them to be removed at pleasure, and exposes a double surface. Glass partitions make the insulation perfect, but troughs of Wedgewood's ware will still be better: this idea was first suggested by Dr. Babington.

Observations.—We applaud the spirit which guided Mr. Children in the construction of his battery, and in the application of it to such useful purposes. His paper contains a great deal of new and interesting matter, and must, we conceive, be acceptable to those who are engaged in similar pursuits. It is unnecessary perhaps to state, that the troughs of the great Voltaic battery of the Royal Institution are of Wedgewood's ware, and that they perfectly answer their intended purposes.

A Memoria Technica for double elective Attractions. By THOMAS YOUNG, *M. D. F. R. S.—Phil. Journ. No. 99.*

DR. Young, for the sake of assisting the memory, has expressed 1260 cases of double elective attractions, in the form of technical hexameters. He refers, for an explanation of the principles on which the arrangement is founded, and for a few particular doubts and exceptions, to a paper in the Philosophical Transactions; and only observes, that each line expresses a column of the table, and that when four substances are mixed, they will so arrange themselves, that the bases will always be united to the respective acids which stand nearest to them.

On the Affinity between Oxides of Carbone and Iron. By DAVID MUSHET, *Esq.—Phil. Mag. No.* 131.

MR. Mushet, after having noticed the utility of investigations of this kind, states the principles on which his experiments were made, viz. the tests afforded by the mutual affinity of iron and carbon, of the existence of each respectively.

The object he first purposes to ascertain is the difference between the oxide of carbon, and the carbonaceous matter in the substances from whence it is procured.

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He then infers, as he found it always to be the case, that two oxides of carbon, with an equal portion of foreign matter, are similar when they revive the same quantity of metallic oxide, but that the qualities of oxide of carbon cannot be ascertained by its reviving power.

Mr. Mushet, by a variety of experiments, has found, that those bodies which yield the most coal or coke and the least ashes, far from being most efficient in the reduction of metallic oxides, are in this respect in general inferior to those which contain a less portion; and it appears, that not only the carbonating power of the oxide of carbon depends on some other cause than the actual quantity of carbonaceous combustible matter contained, but that it also receives in the process of distillation new habits and laws of action, the reverse of what might have been expected. The principle of oxidation was assigned as the cause of this, supposing that the coal or coke, which received the largest quantity of metallic oxide, was most deoxidated; but the contrary of this was in fact found to take place, the most deoxidated reviving *(cæteris paribus)* the least quantity. Mr. M. conceives that the experiments which follow warrant the conclusion, that this inverse ratio of carbonisation proceeds entirely from an approximation to the state of diamond, produced by a denser arrangement of parts from a loss of oxygen.

The methods pursued in these experiments were as follow:—50lbs. of oxide of iron, of the same quality, and similar in every respect, were prepared. The crucibles used were of clay, and sitted with ground covers; when heated to redness, the mixture was introduced, covered, and placed in the assay furnace: by these means moisture, that otherwise would have been present and injured the charcoal, was expelled.

Fifteen grains of oxide of carbon (including 1.5 of ox. of carbon contained in the paper in which the materials were placed) were generally put to 200 of ox. of iron.

In the comparative experiments between the raw and coked materials, a quantity of the former was used sufficient to produce 13 grains of ox. of carbon. In all these experiments the greatest attention was paid to accuracy, and many of them were thrice repeated.

The first class was made on various woods, from which the following are selected:

Exp. 1. Charcoal prepared from walnut, composed of oxide of carbon 96.048, ashes 3.952 = 100. Fifteen of this oxide, and 200 of oxide of iron, produced by fusion 36 grains of metallic iron (= to 18 per cent.); 70 grains of walnut sawdust (= to 15 grains of charcoal) revived 49 grains of iron. Difference between these two products 13 grains; increase = to 6.5 per cent.

Exp. 2. Charcoal from sallow, composed of ox. of carbon 93.865, ashes 6.135 = to 100. Fifteen grains of the charcoal,

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and 200 of ox. of iron, gave by fusion 43 grains of iron (= to 21.5 per cent.); 79 grains of the sawdust of this wood (= to 15 of charcoal) produced by this action on 200 of ox. of iron, 60 grains of iron; increase 17. (= to 8.5 per cent.).

Exp. 3. Charcoal from birch, composed of ox. of carbon 89.681, ashes 10.339 = to 100. Fifteen grains of charcoal, with 200 of ox. of iron, produced by fusion 62 grains of iron (= to 31 per cent.); 90 grains of birch-wood (= to 15 of coal) in sawdust, mixed with 200 of iron, gave 66 grains of iron; increase 4 (= to 2 per cent.).

From these experiments it is evident, that both the quantity and purity of the carbonaceous matter do not depend on the combustible matter contained.

Birch charcoal, which contains 89.681 of combustible matter, revived the largest portion of iron, viz. 62 grains. In walnut the combustible matter was 96.048 grains; iron revived 36 grains.

This contrast shews, that the different carbonating powers of charcoal depend on some unknown principle.

It is ascertained by these experiments, that wood in its natural state has a greater carbonating power than when reduced to charcoal; a curious fact, that may be solved in three different ways:

1st. From the decomposition of the resinous juices of the wood by the ox. of iron, the free carbonaceous matter may combine with the iron or oxygen, and allow the concrete carbon freer action.

2dly. From the larger surface of sawdust.

3dly. From a certain degree of oxidation being required in the charcoal, to facilitate its union with the oxygen of the oxide; and as this degree is greater in raw wood than in charcoal, so are the affinities of the former more powerful and rapid in their action than those of the latter. This by Mr. M. is conceived to be the true cause.

Observations.—As Mr. Mushet's experiments were all made on the same class of bodies, and conducted precisely in the same way, we have only presented two of them in detail to our readers; the results of the others may be seen in the table he has annexed. We are of opinion that they are of no little importance to the various processes of metallurgy, as they point out and demonstrate the superior advantages of wood in its natural state, for the purpose of reduction: this advantage is such in every point of view, that we trust it will attract the attention of the manufacturer, and be of public utility.

Allowing Mr. Mushet due merit for his useful labours, we shall venture to dissent from the theoretical part of his paper.

The deoxygenating powers of carbon and hydrogen are well known, and it is equally well ascertained, that they possess those powers even in a greater degree when combined with each other

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in the form of carburetted hydrogen gas: few are ignorant that wood, during its distillation, evolves a vast quantity of this gas, which we conceive, in its nascent state and high temperature, is fully adequate to produce the phenomenon mentioned.

What appears to us most singular in Mr. Mushet's experiments is, the small quantity of oxide of iron, revived by those charcoals which contain most combustible matter. We have many doubts of the accuracy of these experiments, as those of Messrs. Allen and Pepys will not for a moment allow us to suppose with Mr. M. that charcoals are oxides of carbon, much less that there are different oxides of this substance.

On the Salts of Barytes. By Mr. A. AIKIN.—*Phil. Journ. No. 99.*

FROM a short series of accurate experiments it appears that the carbonate of barytes contains 21.67 per cent. of carbonic acid, and 78.33 of barytes.

Ignited muriate of barytes contains 73.14 per cent. of barytes, and 26.86 of muriatic acid.

Crystallized muriate contains 62.47 per cent. of barytes, 22.93 of muriatic acid, and 14.6 of water.

Sulphate of barytes contains 66.04 per cent. of barytes, and 33.96 of sulphuric acid.

The above results are very nearly the same as those of Klaproth.

Observations.—Mr. Aikin's experiments were conducted with such nice attention to accuracy as to preclude every apparent source of error. To chemists, therefore, they must be highly acceptable, as establishing, by their agreement with Klaproth's results, the uniform composition of the salts of barytes.

Analysis of a mineral Water, near Dudley, in Worcestershire. By Mr. W. WELDON.—*Phil. Journ. No. 99.*

A WATER, about two miles south of Dudley, famous for the cure of scrofula, contains, according to a very detailed analysis, in a gallon, or 231 cubic inches, 5.24 cubic inches of sulphuretted hydrogen, 5.64 of carbonic acid, and 7.1 of azote. It also yielded 323.93 grains of muriate of lime, 96.35 of muriate of magnesia, 38.5 of muriate of alumine, 528.63 of muriate of soda, 0.71 of sulphate of lime, 2.1 of carbonate of lime, 11.12 of carbonate of magnesia, 2.15 of subcarbonate of alumine, 3.36 of subcarbonate of black oxide of iron, 0.475 of subcarbonate of manganese, and 0.2 of silica.

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The water sometimes differs in specific gravity, and composition.

The ingredients here mentioned, are such as occur upon decomposition, but it is probable they are combined in a different order.

Observations.—In the analysis of this water, which exhibits some proofs of ability, we observe a few sources of error.

In estimating the weight of substances obtained by analysis, it is essentially necessary that they should be uniformly dried at known temperatures; for without this precaution it is impossible that the result can be accurate. In this particular Mr. Weldon is deficient.

We presume, also, that he errs in estimating subcarbonate of alumine as an ingredient in this water. We think no such combination is known; and it seems evident that the error must have arisen from subcarbonate of soda, used as the precipitant, and which, in consequence of its affinity, adheres, with great obstinacy, to the alumine.

No notice is taken in the estimates, of the oxygen which the water was found to contain: and azote and atmospheric air appear to be very improperly considered as synonymous.

On a new Method of detecting Arsenic. By Mr. Joseph Hume.—Phil. Mag. No. 131.

The utility of accurate tests for discovering the presence of arsenic, is obvious. The methods already known, though adequate in particular cases, are yet perhaps deficient when the arsenic is in a minute quantity. The method which Mr. Hume considers to be superior to any other, he elucidates by the following experiment:

One grain of white oxide of arsenic, with the same quantity of carbonate of soda, is to be boiled in a silver vessel, with ten or twelve ounces of distilled water; and by the infusion of a few drops of nitrate of silver an immediate precipitate of a bright yellow colour is produced. Mr. Hume observes, that either potash or lime-water may be substituted for carbonate of soda; but that he is inclined to give the preference to the latter.

Observations.—We are sorry to say, that our opinion of the superior excellence of this chemical reagent, to detect the presence of arsenic diffused in a minute quantity through the fluids of the human frame, differs greatly from that of the proposer; he appears to have forgotten the menstruum in which the poison acts in the body, and in which it is still held in solution (supposing the portion to be so small as to escape the notice of the common tests): when examined, this menstruum is not only complex, but

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it even universally holds in solution that which, above all other things, most tends to make the test of no utility, and even a source of error, namely, muriate of soda, which, as it is well known, instantly precipitates nitrate of silver: the precipitate thus formed, might alter the appearance, or conceal the presence of a small quantity of arsenic; and if the fluid examined, was really free from arsenic, the muriate of silver might, by those who are quite ignorant, or not well versed in chemistry, be mistaken for it.

Sulphate of copper, which Mr. Hume considers as a less decisive test than nitrate of silver, is not liable to these objections; and when the poison is taken in a sufficient quantity to prove mortal, it is detected by the sulphate in a sufficiently distinct manner.

Neither do we see that the analyst can put much confidence in, or derive much advantage from, the use of Mr. Hume's method. For it is evident, that when acids are the solvents of the arsenic, nitrate of silver is both useless and fallacious in its detection.

On the Identity of the Base of Charcoal with Hydrogen, or its Base. By J. New, *M. D.*—*Phil. Jour. No.* 101.

Some years ago Dr. New made various experiments and observations which led to the opinion, that "*charcoal and hydrogen are modifications of the same substance*; *or that hydrogen is the base of charcoal.*" These experiments, however, were not committed to writing, and they are now forgotten.

On perusing some experiments of Mr. Berthollet, jun. on sulphur and charcoal (Annales de Chimie, vol. lxi. p. 127), Dr. New conceived that his opinion was almost established by the French chemist, and thought this a favourable opportunity to announce it; not with a view of claiming any priority of discovery, but merely as a hint to inquirers.

Observations.—This opinion of Dr. New is very extraordinary, and, being unsupported by a single fact, it can be considered only as vague and idle speculation, to which no merit can be attached.

That experiments, from which the identity of charcoal and hydrogen could have been fairly inferred, should be neglected, and entirely forgotten, is highly improbable.

Berthollet's experiments merely prove, that sulphur and charcoal contain hydrogen.

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A numerical Table of elective Attractions, with Remarks on the Sequences of double Decompositions. By Dr. THOMAS YOUNG. —*Phil. Trans. Part I.* 1809.

MANY attempts have been made to represent elective attractions by numbers; but they have been confined within too narrow limits to be of any use, and have been hastily abandoned.

In some cases, the presence of a fourth substance, besides the medium in which the two salts are dissolved, may influence the precise force of the attraction; but there is reason to think that these cases are rare.

Dr. Young has given in the original paper, which must be referred to for particulars, a series of numbers which agree sufficiently well with all the cases of double decomposition, fully established, excepting about 20, out of 1200 enumerated by Fourcroy. The same numbers agree in general with the order of simple elective attractions: the common tables of which are certainly imperfect if considered as indicating the attractive forces, independent of other circumstances.

The facts, however, may be stated, independently of any hypothesis, and without the necessity of future alterations to make room for the affections of other substances; and which will also enable us to compare, upon general principles, a multitude of scattered phenomena, and to reject many which are considered as probable or doubtful. This arrangement depends upon the supposition that the attractive force which tends to unite any two substances may be represented by a certain constant quantity. Whence it may be inferred that there is a sequence in the simple elective attractions, an agreement between the simple and double elective attractions, and a continued sequence in the order of double elective attractions, so that to represent the latter sequence, we may place between any two acids, the different bases in such order that any two salts resulting from their union, shall always decompose each other, unless each acid be united with the base nearest to it. For example—*Sulphuric acid*: barita, potass, soda, ammonia, strontia, magnesia, glycina, alumina, zirconia, lime. *Phosphoric acid*: for the same reason, the acids may be arranged in a continued sequence between the different bases.

Two tables of this kind are given in the original, with notices of the apparent exceptions.

The orthography adopted by the chemists should be altered, because there are certain rules of etymology which no modern author has a right to set aside. The English have adopted the Roman alphabet, and write Greek after the Roman manner, υ being written by a *y*, and the Greek diphthong ου by an *u*, the Romans having no such diphthong as *oy* or *ou*. The French some-

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times use *ou*, which may be defended in their language, as their *u* and *ou* are probably pronounced exactly the same as the Greek *υ* and *ου*. Hence glycine, or glycina, should be adopted, instead of glucine.

Barytes, as a Greek word, with the accent on the first syllable, signifies weight: as the name of a stone, accented upon the second syllable, it must be written barites, and the pure earth may be called barita.

Yttria is altered in the tables to itria, because no Latin word begins with a *y*.

Observations.—We regret that the want of room obliges us to omit the excellent tables given by Dr. Young, whose usual industry and abilities have been exerted in the construction of them.

Analysis of bitter Spar. By Mr. Bucholz.—*Journ. de Phys. July* 1807.

Bitter spar (or rhombspar of Jameson) contains 28 per cent. of lime, 25.5 of magnesia, 48 of carbonic acid, and 1.5 of oxide of manganese, with some traces of iron; 2 per cent. being lost in the operations.

Observations.—The excellent investigations of Mr. Descostils on sparry iron ore, throw considerable light on the very confused state in which the native carbonates of lime, magnesia, iron, and manganese are at present. His paper is contained in the present number.

On detonating Silver. By Mr. Figuier.—*Ann. de Chim. No.* 187.

As some account of detonating silver had been already published in the Annales de Chimie, the editors only insert such parts of Mr. Figuier's memoir as are different from what they had already given.

Detonating silver fulminates when touched by a hard substance, even while covered by the solution in which it is formed; when dried, it detonates by the mere friction of the sharp edge of a card.

It is not decomposed by dilute sulphuric acid, unless it has been previously dissolved in water.

Caustic potash changes its colour to a red, or deep grey, but

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without destroying its power of detonation; but if the potash is left on it for some time, only a slight decrepitation is produced, which seems to arise from the particles that have not yet been acted upon by the potash.

Observations.—The preparation of this detonating silver is related in our last volume, page 69.

On the Pollen of the Nut-tree; on suberic Acid; on some unknown Crystals; and on the Blackening of Muriate of Silver by Light. By Prof. Link.—*Ann. de Chim. No.* 186.

The pollen of the nut-tree differs greatly from that of the date-tree, analyzed by Fourcroy and Vauquelin. It contains a quantity of tannin, a resin, much gluten, and a little fibrine; it consists in part of an animal substance.

Every thing obtained by Bouillon-Lagrange from cork, by nitric acid, has been obtained from elder pith, treated in the same manner; except that this left no residuum. As suberic acid is yielded by paper, it is probably furnished by all vegetable membranous substances.

Crystals are observable by a microscope in the cellular tissue of the roots of œnothera biennis. They are insoluble or nearly so in water, alkohol, and many acids; even sulphuric acid acts very weakly on them; nitric acid seems to dissolve them best.

Muriate of silver is not rendered black by a current of air in the dark. Berthollet observed it to blacken by a current of air: he says, the light acts by taking away part of the muriatic acid; but it takes a black colour even if immersed in muriatic acid.

On a Species of Manna, or Sugar, from Rhododendron Ponticum. By Mr. Bosc.—*Ann. de Chim. No.* 187.

Fourcroy and Vauquelin observed some years ago, that rhododendron Ponticum transudes a concrete manna or sugar from the receptacle of its flowers.

This sugar is in grains, appearing and tasting like pure sugar-candy, but it leaves an acerbity behind it; some of the grains are more than two millimetres (8-100ths of an inch) in diameter. It is not found on plants that grow vigorously. Those planted in pots, and sheltered from the night dew and sun, furnish the most, as the sugar is melted by the sun and dew.

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On the Decomposition of Salts by Volta's Pile. By Messrs. RIFFAULT *and* CHAMPRE.—*Ann. de Chim. No.* 187.

THE experiments related in the two memoirs, of which Mr. Descostils gives an abstract only in the above work, are nearly the same as some of those mentioned by Mr. Davy, in regard to the passage of the constituent parts of salts to the respective poles by which they are most strongly attracted; it is needless therefore to repeat what is already well known here. The following circumstances, however, seem to have some novelty to recommend them.

The action of the pile on nitrate of soda is most efficacious when that salt is placed in the positive tube; and the acid liquor obtained threw down sulphate of lime from the muriate of that earth; but it is doubtful whether it did not arise from some impurity.

A solution of muriate of lime, decomposed on the negative side, let fall its lime without any regular figure; but if the muriate was placed on the positive side, the lime was carried over and deposited upon the negative wire, and in the connecting syphon, in crystals; these crystals were found to be straight hexaedral prisms, and to be pure lime.

The energy of the positive current is superior to that of the negative.

A solution of nitrate of lead placed in the positive tube, and water in the negative. The water became turbid, rendered syrup of violets green, metallic lead was deposited on the wire, and a dirty white oxide of lead fell down, which by its mixture with caustic soda exhaled a very perceptible smell of ammonia. The solution was not rendered turbid, it was more acid than the common nitrate of lead, the wire became covered with a black incrustation, shining like sulphuret of lead. This substance yielded, by the blowpipe, the smell of nitric acid, and produced oxymuriatic acid by being treated with common muriatic acid: Vauquelin, on examining it, considered this substance as an oxide of lead with excess of oxygen, from the gas disengaged by the positive wire.

When nitric acid was subjected to the pile, no ammonia was observed on the negative side, but the acid became green, which colour extended to the lower part of the syphon at that side. So that it is probable oxygen was disengaged, and the nitrous gas formed in consequence was absorbed by the acid. The water on the positive side became strongly acid.

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On a new Metal; on Turquoise; and on schorliform Beryl. Letter of Mr. GEHLEN.—*Ann. de Chim. No.* 186.

DR. John, of Berlin, has found, that when sulphuric acid was distilled upon grey ore of manganese from Saxony, a volatile metallic acid came over and combined with the solution of potash in the receiver, forming a crimson solution.

This red liquid gave a brown maroon-coloured precipitate with gallic acid or infusion of galls. It was only changed lemon yellow by prussiate of potash, neither did it yield any precipitate with the carbonates. On being heated with alkohol, it became green, emitted an ethereal smell, and the carbonates then threw down a brown oxide soluble in muriatic acid.

Dr. John has lately analyzed turquoise, from Pischapur, near Korassan. This specimen yielded 73 per cent. of alumine, 4.5 of oxide of copper, 4 of oxide of iron, and 18 of water; 0.5 being lost in the operation. This analysis agrees with that of Lowitz, and shews that there are two species of stones confounded under the name of turquoise. This of Dr. John might be called calaïs, a name used by Pliny.

Mr. Bucholz has found the schorliform beryl of Bavaria to be a true beryl, as it contains 12 per cent. of glycine.

Continuation of Experiments on Bile or Gall. By Mr. THENARD. —*Journ. de Phys. Oct.* 1807.

THE former experiments of Mr. Thenard related only to the gall of oxen, and were abstracted in our last number, p. 113: the present relate to the gall of other animals, or of the human subject.

The gall of the dog, sheep, cat, and calf, is similar to ox-gall. They are greenish yellow, and bitter. On evaporation, they yield a deliquescent extract, soluble in water, yielding a thick smoke on calcination, and leaving soda; phosphate, muriate and sulphate of soda, phosphate of lime and oxide of iron. Acids throw down yellow matter and some little resin; acetate of lead with a slight excess of oxide throws down a good quantity of resin, and the supernatant liquor being filtered, and all the lead separated by sulphuretted hydrogen, there is left picromel in large proportion, mixed with a little acetate of soda.

The gall of the hog is a true soap; it does not contain albuminous matter, nor animal matter, nor picromel, but only a large proportion of resin, with soda and some other salts not yet determined. It is entirely decomposed by acids, even by vinegar.

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The gall of fowls, capons, turkeys, and ducks, is very similar to that of quadrupeds; but it contains a great quantity of albuminous matter; the picromel is not sweet, but very sharp and bitter; very slight traces of soda are observable, and common acetate of lead does not throw down any resin.

The gall of the ray and salmon is yellowish white; it yields by evaporation a very sweet and somewhat sharp matter, and does not appear to contain any resin.

The gall of carp and eel is very green, very bitter, very slightly or not at all albuminous, and contains soda, resin, and the sweet but sharp matter, like that of the ray and salmon, and which is probably picromel.

Although there are persons who frequently throw up considerable quantities of bile without the previous use of emetics, no opportunity has been offered to examine it: of course that of the dead subject alone can be here considered, but it was taken from very fresh bodies.

Human bile is most commonly yellowish brown, sometimes green, and at times almost colourless. It is not very bitter. It generally has some yellowish matter suspended in it, which is sometimes so abundant as to render it grouty. Being heated to ebullition, after it was filtered, it became turbid, and smelt like white of egg. On evaporation, it yielded 1-11th of a brown extract, which being calcined, yielded the same salts as extract of ox-gall.

Acids throw down from human bile albumen and resin, which may be separated by alkohol: 1 gramme of nitric acid, at 25° hydr. saturates 100 of human bile; common acetate of lead changes it into a pale yellow liquid, which does not contain picromel, but only some traces of an animal matter that could not be distinguished.

1100 parts of human bile contain 1000 of water, and the remaining 100 parts contain a very variable quantity of insoluble yellow matter swimming in the bile, from about 2 to 10, and some traces of yellow matter dissolved in the bile itself; also 42 of albumen, 41 of resin, 5 6 of soda, and about 4.5 of phosphate of soda, sulphate of soda, muriate of soda, phosphate of lime, and oxide of iron.

The yellow matter is insoluble in water, oils, or alkohol; soluble in alkalies, from whence it is thrown down in greenish brown flakes by acids. Muriatic acid scarcely acts upon it, but changes it to green brown; it is therefore perfectly similar to that of ox-gall.

The resin of human bile is yellowish, very fusible, very bitter, but less so than that of ox-gall; and it is perfectly similar in respect to the action of alkohol and the alkalies upon it.

Diseases of the liver must greatly alter the bile; when that organ changes into fat, the bile becomes less resinous, and when

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the fat constitutes 5-6ths of the liver, the bile in five instances was almost entirely composed of albumen; in a sixth it contained a very little resin.

On the Calculi of the Gall-bladder of Oxen.

These calculi are usually said to be composed of inspissated bile; but on washing them with water, it appears that they contain only about one per cent. of that fluid interposed between their particles.

The washed gall-stones have no smell nor taste; they are of a fine yellow colour, and have been used in painting, but the colour gradually turns to a brown, as do the stones themselves when exposed to the air and light, but more slowly. They are not changed by heat until the retort begins to be red; they then swell up, yield water, oil, elastic fluids, carbonate of ammonia, and a compact charcoal, which on incineration affords 1-6th of phosphate of lime.

Although they tinge water of their own colour, it does not yield 1-300th of its weight of solid residuum. They are equally insoluble in alkohol and oils. Caustic alkalies dissolve them; the yellow solution is precipitated in green flakes. Muriatic acid has a slight action upon them, and turns them green: hence these gall-stones are evidently composed entirely of yellow matter.

The gall-stones of the human subject have been examined by many persons. They are well known to be fusible by a slight heat, soluble in alkalies, as also in fixed and essential oils. Poulletier de la Salle shewed that they were very soluble in boiling alkohol, and deposited in brilliant flakes on cooling. Fourcroy has since described them more accurately, and remarked that they were analogous to those animal matters that were changed into fat by putrefaction.

Mr. Dupuytren furnished more than 300 human gall-stones, and of these a few were formed of white brilliant laminæ, and composed entirely of adipocere; the greater part were formed of yellow laminæ, and contained from 88 to 94 per cent. of adipocere, and from 12 to 6 of a colouring substance; some were rendered green externally by bile, and were similar to the last internally; several were covered with a blackish brown crust, containing very little adipocere, but were similar to the yellow ones internally; sometimes the black substance was in the centre, and the yellow substance formed the crust; lastly, 2 or 3 were black-brown throughout, without any crystalline appearance. All the coloured gall-stones had bile adherent to them, which might be separated by water.

Two calculi found in the human intestines were also found to be composed of grey and yellow laminæ of adipocere.

The colouring substance of these calculi, when yellow, appears to be the yellow matter of the bile; but when blackish brown, it is probably the same matter altered, so that carbone becomes pre-

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dominant; in the same manner as the gall-stones of oxen become brown by exposure to the air.

These calculi are formed by successive deposits round a centrical nucleus; but as those of the ox are composed only of yellow matter, it is evident that there must exist some circumstances in which this substance is precipitated from the bile, while, on the other hand, those circumstances do not separate the other principles of the bile. Now the yellow matter is insoluble in water, and only retained in solution by means of soda, of which the bile contains but little, so that it is likely that the yellow matter may often be formed in excess, and thus deposited. The resin of the bile is the only other principle insoluble in water, but this is so strongly combined with picromel and soda, that it cannot be separated by the strongest acids.

But the adipocere of human gall-stones is not contained in the bile, and therefore it must either be formed in the liver, and deposited as soon as it is formed, or the resin of the bile is in certain circumstances changed into adipocere.

The gall-stones of other animals, except the hog, are probably similar to those of the ox; and are not likely to contain adipocere, unless perhaps those of birds.

It is said that gall-stones are not found in oxen in summer, as they disappear when those animals are fed with green food; but this is improbable, considering the difficulty of dissolving the yellow matter of which they are composed. Besides, the greatest number of butchers do not examine the gall-bladders in summer, and affirm that the gall-stones are very scarce even in winter; they have, however, been found in two gall-bladders in summer, so that the utmost that can properly be affirmed is, that they are seldomer formed in summer than in winter.

It is equally unlikely that gall-stones of the living human subject should be dissolved by the internal use of ether mixed with oil of turpentine, as it can only be taken in very small doses; besides, the ether must necessarily evaporate, and at all events it would scarcely arrive at the gall-bladder, and even when there could exert but little action upon the stones. It is therefore probable that the good effects of this medicine depend upon its facilitating the passage of these stones through the intestines, especially as the two persons that Mr. Guyton speaks of as cured by this medicine voided the calculi by stool soon after the use of the medicine.

Experiments upon Indigo. By Mr. CHEVREUL.—*Journ. de Phys. Oct.* 1807.

THESE experiments were undertaken by desire of Mr. Vauquelin, to determine the nature of the purple fume yielded by indigo when exposed to heat.

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Indigo by distillation yields water containing ammonia; sulphur probably united with oily hydrogen; a thick brown oil containing carbonate and acetate of ammonia; prussiate and hydroguretted sulphuret of ammonia; a purple substance crystallized in silky tufts in the dome of the retort; a very bulky charcoal yielding prussiate when calcined with potash; and gases which were not examined.

The purple matter obtained by distillation was sullied with oil. To obtain it in purity, a few grains of indigo were exposed to heat in a close-covered platina or silver crucible placed among the coals: the purple matter crystallizes in needles in the middle part of the crucible.

Hot water, after having exhausted Guatimala indigo of all that it could dissolve, was yellowish; it yielded on distillation an ammoniacal product, and let fall a greenish powder that turned blue by the contact of the air, so that it was probably indigo at a minimum of oxidizement. Some time afterwards the water let fall a green matter, which is little soluble in water, but dissolves by help of the alkalies; the solutions are reddish, and on saturating the alkali with an acid they return to a green colour; and if the solutions were concentrated, the green matter precipitates. Alkohol takes up this green matter, and becomes red; but when spread out thin, or mixed with water, it appears green. If after the precipitation of this green matter from the concentrated solution alkohol be added, a yellowish brown, bitter, astringent matter is separated, which burns on the fire, and exhales the smell of empyreumatic vinegar. The alkohol becomes reddish by taking up a combination of the green matter with ammonia. Indigo lost 12 per cent. by this treatment with water. Both the indigo ad minimum and the green matter seem to be taken up by the water, on account of their being combined with ammonia.

After the indigo had been exhausted by water, alkohol separated green matter, a red matter, and indigo ad maximum. The insolubility of the green matter when water was employed, is owing to the ammonia not being sufficient to render the whole soluble, and also to the attraction of the red matter. This red matter is very similar to the green, but its colour is not changed by alkalies or acids, and it is precipitated from them in red flakes. The treatment with alkohol separated 30 per cent. of the original indigo.

From the remainder of the indigo, muriatic acid separated 10 per cent. of the original weight, of which 2 were iron mixed with alumine, 2 were carbonate of lime, and 6 were red matter. The indigo that still remained was of course 45 per cent. of the original weight, deducting 3 per cent. for the silica it was found to retain.

Other kinds of indigo did not yield exactly the same results. In most the green matter was of a fawn colour, it became red by

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alkalies, but acids did not turn it green. A very dark blue specimen, in square heavy cakes, yielded no indigo ad minimum; the ashes of this kind contained more iron than above stated, and also some magnesia. A specimen, said to be from Bengal, yielded 1-20th of indigo ad minimum, and its ashes contained a little sulphate of lime. In some specimens were found traces of phosphate of lime.

The colouring matters that are mixed with pure indigo are probably modifications of that substance.

Pure indigo has a violet tinge; it gives a beautiful blue solution in sulphuric acid. It yields by heat a purple smoke, that condenses in acicular crystals, which are perfectly similar to indigo purified by solvents, except that it does not contain any of the earths retained by the other. If a small quantity of this pure indigo is exposed to a sudden heat, it is volatilized without alteration; but if slowly heated, some part of it is decomposed. This volatilization of a body containing so much carbone as indigo does is very remarkable.

Pure indigo is but slightly soluble in alkohol, and the greatest part of what is dissolved separates from that solvent after some time; if the indigo retains any of the red matter, the solution is permanent.

Neither muriatic acid, nor caustic potash, have any action on it in the cold; but on applying heat they become yellowish, as some of the indigo seems to be decomposed.

Indigo is disoxidized by sulphuretted hydrogen, as has been shewn by Vauquelin; and thus it not only appears, that part of the oxygen in indigo has a kind of separate existence from the other principles, so that it can be separated without destroying the colouring matter; but also that carbone is not the cause of the dyeing property of this drug, as it loses its colour even when it retains most carbone. This action of sulphuretted hydrogen upon indigo is similar to its effects upon metallic substances, and assimilates indigo to them.

Common indigo, moistened with water, is slowly decomposed, and emits an ammoniacal and slightly sulphureous odour. It then contains a portion of disoxidized indigo, held in solution by ammonia.

Glutinous matter mixed with indigo did not emit any gas in two months, but the liquor became green, and contained a large proportion of disoxidized indigo, as also carbonate of ammonia.

Observations.—In the third volume of Nicholson's quarto Journal is an account of a metalliform sublimate obtainable from indigo by distillation, which appears from the present experiments to have been pure indigo.

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On the Acetate of Ammonia, commonly called Spiritus Mindereri. By Mr. PHIL. ANTH. STEINACHER.—*Jour. de Phys. and Ann. de Chim. Nov.* 1807.

A PROPERLY saturated acetate of ammonia cannot be obtained when weak acid is employed, and the mixture made without heat. Carbonate of ammonia was added to the first half of vinegar distilled in a glass retort, and no perfect saturation could be obtained although the mixture was kept well stirred, and the acid or alkali was added at long intervals of time; nor was a gentle warmth of any benefit; so that it was necessary to add a slight excess of acid to fix the ammonia.

Glacial acetic acid diluted so as to have the same specific gravity as common distilled vinegar, was used with no greater success: neither could a saturated mixture be obtained by using vinegar that had been exposed to the temperature of only 5° Réaum.; and then had been drawn off to an half by distillation, so that its specific gravity was double that of distilled vinegar.

Indeed acetate of ammonia requires for its perfect saturation, concentrated acid, and alkali.

Water saturated with ammoniacal gas, at the temperature of melting ice, poured upon rectified glacial radical vinegar, emitted much heat and white vapours; but on being cooled, the point of saturation was soon found. Glacial acetic acid, diluted with five times as much distilled water, also produced a perfect saturation with caustic ammonia.

Lassone proposed the use of radical vinegar, to prepare ammoniacal acetate; but in fact, when radical vinegar distilled from verdigris is combined with the volatile alkali, it always exhales a very disagreeable empyreumatic odour, especially when diluted with water, which probably arises from an empyreumatic oil contained in the radical vinegar.

On repeating Lassone's process of subliming equal parts of sal ammoniac, whiting, and rectified radical vinegar, an empyreumatic white salt was obtained, which turned paper dyed with red roses, of a green colour, although sulphuric acid disengaged vapours of radical vinegar.

It was singular, that this salt, after having been kept for a year in a well-closed bottle, did, on opening the bottle, become of a deep red colour; the empyreumatic oil being suddenly carbonized by the contact of the air, as happens to oil of Dippel, which frequently blackens, on the bottle in which it has been kept for some time, being opened.

The residuum of this sublimation contains ammonia with excess of acetic acid.

Saturated acetate of ammonia neither volatilizes entirely, nor gives out its acid on evaporation, as has been stated; but it yields ammonia by being heated, and the residuum has an excess of acid.

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A pound of ammoniacal acetate, prepared from crystallized radical vinegar diluted with five times as much water and caustic ammonia as highly impregnated as possible, was distilled at the temperature of 25 or 30° Réaum. (88° or 99° Fahr.), until one half had come over. The distilled liquor had a sharp taste, it rendered tincture of red roses green, and formed white clouds with the vapour of muriatic acid. The remaining half was distilled at a boiling heat, until only four ounces were left: what came over was still alkaline. The residuary four ounces being plunged into a bath of common salt and ice, yielded a mass of red, empyreumatic, sour-tasted crystals.

In this decomposition there is a concourse of two attractive forces, that of ammonia for caloric, and that of acetate of ammonia for an excess of acid; so that saturated acetate of ammonia only parts with its ammonia, until the residuum acquires such an excess of acid as to become fixed; of course, by adding a large quantity of acid to an imperfect acetate made with a weak acid, it may be rendered so fixed as not to give out any ammonia.

Four pounds of Orleans vinegar were poured upon an ounce of carbonate of ammonia: one half was then distilled off, by a boiling heat; the distilled liquor was very acid, and did not contain any ammonia. When only eight ounces were left in the retort, the residuum was yellow and very acid; from which, another ounce being evaporated in an open vessel, it exhaled the odour of radical vinegar, and the liquid became of a dark amber colour, which being exposed to a cold of 6° — 0 Réaum. (19° Fahr.), congealed into a mass. This mass having been drained for a night, was pressed between blotting-paper, and thus there were procured six drachms of very white ammoniacal acetate of an agreeable saline smell and taste, which slightly reddened tincture of litmus. It did not melt in the heat of summer.

Acetate of ammonia is very easily separated into ammonia and the acetate with excess of acid; for though, on dipping white blotting-paper, tinged with infusion of red roses or litmus, into a saturated acetate, its colour was not changed; yet, on exposing it to the air for a quarter of an hour, the litmus paper became very red, and the red rose paper was not changed.

Acetate of ammonia perfectly saturated, in a well-closed and full bottle in a cool place, kept for a year without alteration; but another portion kept in a bottle, of which it filled only one third, and exposed to the various temperatures of the air, became slightly acid.

Many processes have been given for preparing acetate of ammonia; the preceding experiments evidently shew that they may be rendered more perfect. The following is a very simple and good method:

Thirty parts of the first half distilled off from a good white

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wine-vinegar, is to be mixed with one part of the first portions of very dry and newly sublimed carbonate of ammonia, and the mixture boiled gently, until ten parts are consumed. The liquor becomes yellowish and subacid. Being cooled, a small quantity of ammoniacal carbonate must be added, to saturate the acid; after which, it must be kept in a well-stopped bottle and a cool place.

The first twelfth part that passes over when good wine-vinegar is slowly distilled in a glass retort, although as colourless as water, is charged with colouring extractive matter; which serves to distinguish strong acetic acid, obtained by the freezing of distilled vinegar, from the acid obtained by the distillation of acetates.

When the acid obtained by freezing distilled vinegar, is poured upon minium, the oxide is first lowered to its minimum of oxidizement by the extractive matter, and then totally dissolved.

The acid obtained by distillation of the acetates, not containing extractive matter, but an empyreumatic oil, changes minium partly into a brown oxide at the maximum of oxidizement, which is not dissolved; at the same time another portion of the oxide is lowered to the minimum, and taken up by the acid.

The insoluble oxide thus produced by radical vinegar, is similar to the brown oxide obtained by Proust, on using nitric acid; and by Vauquelin, on using oxymuriatic acid.

Observations.—The author of this paper is certainly mistaken in supposing that chemists have always stated ammoniacal acetate to yield its acid by distillation. Dr. Bryan Higgins has already shewn, in his Treatise on acetous Acid, that when a solution of acetate of ammonia in water is distilled, ammonia first passes over, then the acid; and lastly, some of the salt itself; but that no such decomposition takes place when dry acetate of ammonia is sublimed by a moderate heat.

The experiments in the latter part of the paper shew a real difference in the properties of the acetous and acetic acids, as the former dissolves minium entirely, but the latter only takes up part of it, and leaves the remainder in the form of brown oxide of lead.

On volcanic ferruginous Sand. By Mr. Louis Cordier.—*Jour. des Mines, No.* 124.

Ferruginous sand is found in the neighbourhood of volcanoes; but most commonly mixed with fragments of lavas, and grains of feldspar, pyroxene, mica, amphibole, black garnet, and amphigene, to which are sometimes added corundum, zircon, spinelle, and silico-calcareous titan; of which last stone the somelins is a variety.

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The grains of this sand seldom attain four or five millimetres in diameter; they are angular, and sometimes octaedral, perfectly opaque and black. They do not scratch glass, are brittle, and yield a black powder. Their surface is metallic, passing to vitreous, but sometimes dull; the fracture is smooth, commonly shining and perfectly conchoidal, without any appearance of laminæ. They are strongly attracted by the magnet, and sometimes have poles. Their specific gravity is 4.590 to 4.890. Infusible, but yield with borax a black enamel, the small fragments of which are blackish green, and semitransparent. They are not found in a decomposed state. They are totally soluble in muriatic acid.

This sand has been confounded with the fer oxydulé of Haüy; and might be mistaken for the fer oligiste of the same author; but the above characters are sufficient to distinguish it.

The ferruginous sand of Teneriffe contains 79.2 per cent. of oxide of iron, 14.8 of oxide of titan, 1.6 of oxide of manganese, 0.8 of alumine, and a trace of chromic acid.

That of Puy in the department of the Haut Loire, contains 82 per cent. of oxide of iron, 12.6 of oxide of titan, 4.5 of oxide of manganese, and 0.6 of alumine.

That of Niedermennich, near Andernach on the Rhine, contains 79 per cent. of oxide of iron, 15.9 of oxide of titan, 2.6 of oxide of manganese, and 1 of alumine.

These analyses were confirmed by the examination of 27 other specimens from various countries; and this volcanic sand ought therefore to be considered as a peculiar species, to which the name of *fer titané* (titanized iron) might be assigned.

The banks near Andernach, from whence the ferruginous sand of Niedermennich was taken, extend to the base of two magnificent craters, Bellenberg and Hochsimmern. These craters are perfectly insulated, and rise above the surrounding volcanic country. Their form is that of a cone obliquely truncated, terminated by a deep fissure on one side. They are above 200 metres (yards) in height. They are totally formed of common red scoriæ, crumbling in the hands.

These craters have escaped the observation of Ferber, Collini, and other mineralists, who have examined this country, probably because they are out of the common road. That they are really extinct volcanoes, is evident from their description, although some German mineralists (Werner and his scholars) suppose, that whenever extinct volcanoes are spoken of, the observer is mistaken, and gives that name to common basaltic caps, such as are found in their country.

Observations.—This is only the first of a series of observations on different volcanic substances. In the third volume of this work, page 326, may be found a short notice of what is here given in detail.

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The ferruginous sands here spoken of, differ from the menachanite from Cornwall, by containing a much larger proportion of oxide of iron, in consequence of which, they are more strongly attracted by the magnet. Menachanite also contains silica in a notable proportion. This difference of composition certainly entitles these sands to form a separate species; but the name of fer titané is equally applicable, both to menachanite and these sands, and therefore equivocal, especially as the oxide of iron is superior in quantity to the oxide of titan even in menachanite.

We take this opportunity of announcing, that Dr. Wollaston has lately found the columbium of Hatchett to be the same metallic substance as the titanium of Klaproth, or menachan of Werner.

On sparry Iron Ore. *By Mr.* COLLET DESCOSTILS.—*Jour. des Mines, No.* 124.

In a former memoir (see Retrospect, vol. ii. p. 334), the presence of magnesia was shewn to be the cause of the difficult fusion of some sparry iron ores. It has since been found, that when these ores are melted in a crucible lined with charcoal powder, the magnesia does not vitrify, and hinders the fused iron from uniting into a button.

The usual process for rendering these infusible ores more tractable, is, to expose them to the weather, which appears to act by abstracting the magnesia: this idea could only be proved by comparative analyses of the same ore before and after the exposition: but as this process is but little used in France, such specimens could not be procured.

Two specimens were therefore taken from the same vein, one of which was not altered; the other was decomposed, so as to be rendered fusible. The recent specimen contained four per cent. of magnesia, and the metals were combined with carbonic acid. The decomposed specimen did not contain any magnesia nor carbonic acid, and the metals were oxidized ad maximum. Five other specimens of fusible ore from different places, yielded similar results. In all cases, the lime was rather more abundant in the altered ore, because of the abstraction of part of the other principles. The muriatic solution of the altered ore yields a siliceous jelly, that seems to indicate the union of silica with the iron; but this jelly has never been observed in the solutions of the unaltered ore.

The same change undoubtedly takes place in ores exposed for a long time to the air and rain. The change has been supposed to arise from the dissolution of the carbonate of magnesia by the rain, augmented by the increasing oxidizement of the metals; or from the decomposition of the sulphate of iron, arising from the pyrites, which are almost always mixed with the sparry ore, as this metallic sul-

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phate is decomposed by the magnesia, and the new-formed sulphate of magnesia is carried off by the rain. Mr. Le Maire has observed an efflorescence of sulphate of magnesia upon a heap of washed ore. It must, however, be confessed, that pyrites are not regularly distributed among the ore, and that they may be found in fusible ore, with all their native brilliancy. The water that had washed some roasted ore at Allevard, which was covered with white efflorescences, did not contain any iron, but only the sulphates of magnesia and lime: this last formed one third of the residuum. As these proportions are not constant, it is probable that some efflorescences consist only of sulphate of lime.

A similar result may be obtained from immersing roasted magnesian sparry iron ore; but if the ore is too much roasted, the magnesia does not decompose the sulphate of iron. The presence of carbonic acid is not so prejudicial.

Several essays were made of a crystal of the iron ore of Elba; one without roasting or pulverizing it; but barely involving it in powdered charcoal, which yielded a perfect button; others had magnesia added to the ore, which was well pulverized, and moistened with oil; and it appeared that fifteen parts of magnesia rendered 100 of the ore refractory. The former experiment shewed also, that the hardness of ore, or its resistance to acids, does not influence its fusion, and that large fragments require only a longer time, but not a greater heat, than small pieces.

As the separation of magnesia is the object in preparing these ores, it may be accelerated by moistening the ore with a solution of sulphate of iron, and after some days washing it with water: or the pyrites contained in the vein may be slightly roasted, and then left to effloresce; but they must be carefully separated from the ore itself, because they would not in this process have sufficient time to be decomposed, and would spoil the iron.

The magnesia that may be left in the ore, is fusible with lime, silica, and alumine; and these earths are found in small proportions in the scoria of sparry iron ore. Oxide of manganese may be substituted for alumine; and this explains a circumstance observed at Allevard, which appears at first to contradict what has been said above.

It is there the practice, when the smelting does not go on well, to add some magnesian (or undecomposed) sparry iron ore; but it is probable, that this addition is to prevent the production of white cast iron, as the grey is the object in this foundery. Too much charcoal is used in this furnace to suppose that the want of it would produce white metal; but the ore itself often contains a large proportion of manganese, which has that effect, even although the metal is exposed to a violent heat for a long time upon a bed of charcoal. Iron alone gives a very brown metal. The magnesian ore they employ is very poor in manganese: by diminishing the proportion of that metal in the charge, and in favouring the com-

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bination of the iron with charcoal, by the magnesia rendering the ore more refractory, the metal is brought to a grey colour. This explication is not, however, certain, because the facts have not yet been observed with proper care.

It would be of great service if the qualities of sparry iron ore could be discovered by external characters, or some easy trials; but neither the texture, fracture, brilliancy, colour, loss of weight by roasting, or specific gravity, nor even the union of several of these characters, yield any indication of the constituent principles of sparry iron ores, notwithstanding the observation in the former paper respecting the ore denominated maillat; for fusible ores are also found composed of large plates, and which contain very little magnesia.

The most simple process is to fuse ten grammes (150 grains) of the ore, previously pulverized and moistened with oil, in a covered crucible, filled with charcoal powder. The essay may be made in a smith's forge, by placing some bricks opposite to the twyer to confine the fire. If this essay produces an earthy greyish mass, friable, and filled with small globules, the ore is very magnesian and refractory. A well-formed button, with a few scoriæ, shews that the ore is fusible, and contains but little manganese; green scoriæ indicate manganese; it must however be observed, that, if the fusion is continued too long, the manganese is reduced, and united with the iron.

The undecomposed ores lose from 31 to 37 per cent. by roasting, the decomposed ores lose 14, which is only water, and grow rather harder. The proportion of magnesia and oxide of manganese is very variable; they each sometimes form 12 per cent. of the ore, and at others are scarcely to be found. A large proportion of the one seems to exclude a large proportion of the other; but the absence of one does not infer the presence of the other; so that the oxide of iron always forms more than half the ore, and sometimes much more, particularly the ores from the Pyrenees, which are easily smelted by the Catalan method.

This method consists in reducing oxide of iron, by a kind of cementation in contact with the burning fuel, and when the greatest part of the iron is reduced, the earthy substances and unreduced oxides are vitrified by the heat being increased, so that the globules may unite. Sometimes the metal obtained, is really excellent steel, and may be forged into bars. This operation is the easier as the ore contains less earthy matter. If the earths are fusible, nothing need be added, but at Baigorry the crude ore contains four per cent. of magnesia, so that a flux must be added; and it is evident that some German sparry iron ores, which contain 12 or 14 per cent. of magnesia, could not be treated by the Catalan method. The scoriæ always contain a large proportion of oxide of iron, and the larger as the earthy parts are more abundant in the ore, whence poor ores cannot be thus treated, while

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decomposed sparry ore, and that of the isle of Elba, are peculiarly adapted for this process. The reduction to a perfectly ductile state by cementation, at a moderate heat, has been shewn by direct experiments made on large quantities by Mushet. Similar results have been obtained from trials of 15 or 20 grammes (230 to 300 grains) of crystals of ore from Elba. This easy reduction of the ore explains the preference given in some refineries to white cast iron, as it probably owes its colour to the degree of oxygenizement only. If the cementation is prolonged, and the heat increased, steel is produced; and for a similar reason, charcoal that burns with difficulty, tends to produce steel, and quick-burning charcoal to produce soft iron, as is well known.

To this paper are annexed 21 analyses of different sparry iron ores, with their external characters, and in some cases their respective fusibility, to corroborate what has been said above of the impossibility of judging concerning the contents of this ore, by its external appearance.

Observations.—It evidently appears, that the carbonates of lime and iron are so extremely similar in their crystallization, and absorb such variable proportions of oxidized manganese and magnesia, that several distinct species have been confounded together. Even Mr. Descostils seems to have fallen into this error, as we conceive the Pyrenean ores which he mentions, are perfectly distinct from the common sparry iron ore.

By comparing the results of the analyses quoted by him, white or sparry iron ore is composed of 50 to 60 per cent. of oxide of iron; ½ to 6 of oxide of manganese, which sometimes extends to 12; 1 to 5 of magnesia, which also sometimes extends to 12; 31 to 37 of water or other volatile substances, and a small quantity of lime which never exceeds ½ per cent. The specific gravity varies from 3.6 to 3.9, water being unity.

The rich spathose ore of the Pyrenees, &c. contains 72 to 86 per cent. of oxide of iron; 1 to 2 of oxide of manganese, which sometimes extends to 6; a very slight trace of magnesia, sometimes totally wanting; 1 to 3 of silica; 7 to 14 of water or other volatile substances; and ½ to 1 of lime, sometimes totally wanting. The specific gravity of only one variety has been noted, and that was 4.027.

Both these species are perfectly distinguishable by the proportions of their principles from brown spar, or magnesian spar; in confirmation of which, Mr. Descostils gives an analysis of a ferriferous calcareous spar (brown spar) from Allevard, which lost 43 per cent. by calcination, and yielded 15.5 of red oxide of iron, 2.25 of brown oxide of manganese, 26.5 of lime, and 11.5 of magnesia.

An analysis of magnesian spar (bitter spath) by Bucholz may be found in the present number.

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On fetid Quartz, Avanturine, saccharoid Felspar, a Variety of Klingstein, the Phosphates of Iron, and phosphorescent Talc. By Mr. P. M. S. Bigot de Morogues—*Journ. des Mines, No.* 125.

The environs of Nantes abound with fetid quartz, which is an integrant part of the granite found there; but neither the felspar, nor the mica that accompany it, are fetid. The fetidity increases as the grains of quartz are larger, and is more perceptible in the external parts of the granite, than in those seated deeper. It is frequently accompanied with crystallized felspar, and rhomboidal mica, and has the same crystalline forms as ordinary quartz.

The fetid quartz has hitherto been always found of a greyish cast, semi-transparent, rather greasy, and strongly phosphorescent. Its fracture is vitreous, passing into conchoidal.

Along with the fetid quartz are found many loose rounded nodules, the external parts of which are brown avanturine, but the internal, neither coloured, nor avanturined, unless the nodule was very small; so that the whole was as it were decomposed. It appears that these avanturines are the extreme of a suite, of which fetid quartz is the other end, the metallic appearance being produced by the escape of the fetid principle, and subsequent oxidizement of the oxide of iron by water introduced through the fissures.

In the same place is also found a new variety of felspar, of an opaque snow-white colour, which appears to be composed of brilliant grains of a shining white, and rather transparent. It is sometimes friable between the fingers, and its powder slightly scratches glass, but it is occasionally harder. It absorbs 0.75 per cent. of water, which sometimes causes it to become oker-yellow. By the blowpipe, the grains agglutinate, and form an enamel not so well melted as that yielded by ordinary felspar. It is not electric by heat, nor phosphorescent by friction. This "*saccharoid felspar*" differs from the granular and argilliform felspar of Haüy, as not being decomposed, nor is it unfusible, like the latter, which has lost all its potash. The saccharoid appearance is in fact produced by hasty crystallization.

Between Porte Saint Pere and Nantes is found an opaque rock, which appears by the magnifying glass to be simple. It is opaque, slightly translucid at very sharp edges, white passing into greenish brown. It is slaty, and its laminæ are of indeterminate length or thickness. The cross fracture is compact, or very slightly conchoidal. Both the fractures meeting together become splintery. It is very hard to break, slightly elastic, and very sonorous. Its specific gravity is 2.625. It gives a clayey smell when breathed upon; scratches copper, but receives a metallic appearance from it. It scratches glass with ease, but does not strike fire with steel. The colour of its powder is rather paler than the mass, and somewhat smooth to the touch; not

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electric by heat; becomes brown and increases in hardness by a red heat, and is then magnetic. It yielded by the blowpipe, a very badly melted black enamel, which is still more strongly magnetic, scarcely fusible with borax, but forms with it a white transparent glass. It differs from hornblende slate by its fracture, colour, and hardness, and approaches towards klingstein. In a geological view it may be arranged with the serpentines.

Blue iron earth appears, from some specimens found near Nantes, to be divisible into two species. The first is a true phosphate of iron, to which probably belongs the specimen found by Dubuisson at Four de Diable, in a granite vein, which was brown or deep blue, and became black in the air. It yielded a blueish white powder. By the blowpipe, this powder turned of its original black-grey, and by a stronger heat yielded a badly melted black enamel. It gave a deep yellow transparent glass with borax. A deep blue variety is found in the lava of Auvergne, and the Isle of France. Both these are accompanied with phosphate of lime. In the department of Jura an ore of this kind is found in grains agglutinated by a shelly marle of a blueish colour. The second species is that found in peat and moist clays, which seems to arise from the decomposition of vegetables, and might be supposed to be a prussiate of iron, but it loses its colour, and afterwards dissolves in nitric acid; and if the solution is evaporated to dryness, the residuum is again soluble in fresh acid. It becomes almost black with oil. By the blowpipe, it first becomes brown, and then melts into a brown enamel, almost black; but with borax it yields a topaz yellow glass. This species is probably phosphated hydrate of iron. To these two different phosphates may be added a third, which may be called either a manganesiferous phosphate of iron, or ferriferous phosphate of manganese; it is found at Chanteloub. This has been shewn by M. Vauquelin not to contain water.

There seems to have been produced from the decomposition of gneiss, a "*phosphorescent talc*," which, when pure, is white, brilliant, and very soft to the touch. It does not stick to the tongue, nor smell clayey when breathed upon. It is very friable, much lighter than green chlorite. By the blowpipe, it neither melts nor changes its colour, but only yields a lively splendour, like that given by fluate of lime. By a stronger heat, the splendour disappears; and again reappears on the stone being heated afresh after cooling. It does not melt, but becomes harsh to the fingers. It yields with soda a white frit, but with borax a semi-transparent, white, and well-formed glass bead. A yellowish clay also arising from the decomposition of gneiss seems to be a variety of this mineral. The natrite of Brongniart greatly resembles this phosphorescent talc, but is more brilliant, of superior hardness, and is fusible with great ease, which last property probably depends on its containing 17 per cent. of potash,

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whereas the phosphorescent talc does not seem to contain any of that alkali. Nacrite is also an original fossil, and not arising from decomposition.

Observations.—These are the most interesting parts of this paper; the remainder either has nothing new, or is merely local.

In the last number we gave some remarks on fetid quartz, to which the present may be considered as a supplement. The origin of avanturine is well explained, and it is probable that the generality of these stones is produced in that way.

The phosphates of iron are ably treated of, and the confusion in which that subject was involved, is cleared up.

The saccharoid felspar and phosphorescent talc are evidently new species, as is also the schistose mineral that resembles hornblende slate.

On the Sulphates of Lime, Barytes, and Lead. By M. Berthier. —*Journ. des Mines, No.* 124.

From a number of experiments, some of which were made by the direct union of the constituent principles, it is shewn that ordinary sulphate of lime contains 21 or 22 per cent. of water, which is so easily got rid of by heat, that it is probable very little or none remains after calcination.

Anhydrous sulphate of lime, and calcined ordinary sulphate, contain 42 or 43 per cent. of lime, and 58 or 57 of acid.

Sulphate of barytes consists of 66.5 per cent. of barytes, and 33.5 of acid.

Calcined sulphate of lead contains 69 per cent. of the metal, and 26 of acid, so that there remain 5 for the oxygen united with the lead; whence it follows that the oxide in the sulphate contains 6.8 per cent. of oxygen; and this nearly agreeing with Vauquelin, who shewed that this oxide contained 7 per cent. of oxygen, strongly corroborates the accuracy of these experiments.

If the sulphuric acid is not really anhydrous in these three sulphates, it is at least in the same degree of concentration. This result appears singular, but it tends to render the idea probable, that salts do not retain water after they have been strongly calcined.

The different mechanical divisions of the natural sulphates of lime do not depend upon the proportion of their constituent parts, as they perfectly agree in that respect.

Oxalate of lime contains nearly 39 per cent. of lime.

Observations.—The ingenious speculations of Dalton are unknown in France, and, therefore, Mr. Berthier could not apply them to the verification of his analyses; nevertheless we are disposed to place confidence in these statements.

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Analysis of a Slate from the Excavations at Port Bonaparte, near Cherbourg. By Mr. Berthier.—*Journ. des Mines, No.* 124.

This rock is of a dirty green, and is very slightly unctuous, and talky. It is of a slaty texture, and there appear between the laminæ, small grains of crystalline quartz. Some of these grains have a foliated fracture, and are probably felspar. These grains could not be separated by elutriation.

It contains blocks of granite, which are sometimes very large and rounded; and it alternates with very well characterized breccia, talky and argillaceous slates.

It was found to contain, 71 per cent. of silica, 15 of alumine, 5 of oxided iron, 2 of magnesia, 0.5 at most of lime, 2.5 of potash, and 3 of water; 1.6 were lost. The potash probably arises from the felspar which the slate seems to contain.

Observations.—This analysis is of very little use, from the admixture of the quartzose, or felspar grains, not being separated, nor their quantity ascertained.

On Haüyne, a new mineral Substance. By Mr. T. C. Baruh Neergaard.—*Journ. des Mines, No.* 125.

This mineral was discovered in the mountains of Latium, near the Lake of Nemi, by the Abbé Gismondi, and described by him under the name of latialite in a memoir, not yet published, but read to the Academy of the Lyncei at Rome in 1803.

As this mineral has since been found at Somma, its name of latialite is improper, and it might be called Haüyne, as a memorial of that French mineralist. Thompson has indeed given that name to a mineral found at Somma, which Haüy himself considers only as a variety of idocrase, and the Germans have given the name of haüit to a mineral which is nothing but an arragonite.

Haüyne has been taken for fluor spar, or spinelle; it has a great resemblance to gadolinite and lazulite. It is of a beautiful sky-blue, which passes into an aquamarine green; its lustre is vitreous; it scratches glass, felspar, and even quartz, but the last very slightly. It is very brittle. Its fracture is glassy with angular fragments. Specific gravity 3.3, or 3.1. Becomes electric by communication, but not by heat. It acquires the resinous electricity by friction when insulated. It is not magnetic. Infusible, and does not lose its colour. It yields a fine topaz yellow glass with borax. It forms a very strong jelly with the nitric, muriatic, or sulphuric acids. It is always accompanied with augite, mica, and, perhaps, amphigene, and is found in volcanic countries.

It contains 30 per cent. of silica, 15 of alumine, 25 of sul-

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phate of lime, 5 of lime, 11 of potash, 1 of oxidized iron, and an inappreciable quantity of sulphuretted hydrogen, so that 13 were lost, which were probably water. Analysis, therefore, shews it has an analogy to lazulite, which also contains silica, lime, and sulphate of lime; but no potash has as yet been found in that stone.

Small blue grains, and even crystals, have been found in the lava and pumice at Andernach on the Rhine, which Cordier thought were spinelle: it would require a larger quantity to determine whether they are such, or haüyne. Mr. Swedenstierna has sent from Sweden to Haüy, as spinelle, an octaedral blue mineral, in carbonate of lime, which has some resemblance to haüyne.

Observations.—We have here a new volcanic gem, in addition to the great variety already discovered in lava; and yet we do not doubt but as great a variety of minerals might be discovered, even in the sands of mountainous countries, if they were carefully examined.

On the Formation in which Anthracite has been lately found. By Mr. TONNELIER.—*Journ. des Mines, No.* 125.

ANTHRACITE has been found by Mr. Omalius de Halloy on the banks of the Meuse, 14 chiliometres (8 miles) below Liege. It is perfectly black, of a very brilliant metallic lustre. It is not altered by being heated to redness. It is found in kidneys sometimes as big as a man's fist, and at others, very small, engaged in a blueish bituminiferous carbonated limestone, along with belemnites, oysters, and other shells. These kidneys are sometimes found in the mass of the limestone, but more frequently their veinstone is white rhomboidal crystals of carbonate of lime, which are not infiltrations, but blend intimately with the limestone. The kidneys also frequently terminate in very fine points, so that they shew no signs of ever having been rolled masses.

Hitherto, anthracite has been principally found in primitive rocks; Mr. Héricart de Thury, who found it in secondary rocks, supposed that it originated from vegetables, but was anterior to the existence of animals; whereas coals arose, he thought, from a mixture of vegetable and animal remains. The anthracite found by Mr. Omalius is in limestone, containing animal remains, which alternate with slates and sandstones, containing beds of coal, and must be regarded as contemporaneous with the coal itself; but it is evidently posterior to the existence of animals with vertebræ, since they are imprinted in the calcareous mass that contains it.

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The coal-field in which this anthracite is found commences on the west about 10 chiliometres (6 miles) from Douay, and passing the Scheld, extends along the Meuse and Sambre for more than 20 myriametres, or 120 miles.

In a subsequent paper, Mr. Omalius himself treats of a singular formation which he has observed to take place at Durbuy, where a hill is formed of the above limestone, and cut down vertically, so that the beds are distinguishable with ease: those on each side of the hill are inclined at an angle of about 60 to 80 degrees. The lowest bed presents the figure of a wedge, the next are inclined, so as to form a summit equally wedgelike, and so on to the top of the hill: but this has been truncated by some cause or other, and the more superficial strata are only applied upon one another, and do not join at top. Although it seems necessary that depositions should be formed upon a horizontal plane, yet these beds seem to be in their original position; for if they had been displaced, the part in which they were bent would, he says, be thinner than the others, whereas it is in fact thicker.

On Blende. By Professor Proust.—*Journ. des Mines, No.* 126.

If blende contained oxygen, it would part with it to carbone or sulphur. Yet yellow transparent blende, heated with charcoal, was not in the least altered, nor did it yield any sulphurous acid. The same blende, heated with sulphur, was neither augmented in weight nor changed in colour.

One hundred and twenty-five grains of oxide of zinc (equal to 100 of metallic zinc), heated with 125 of sulphur, produced 116 of sulphuret; this, heated with fresh sulphur, increased to 118; but the zinc could not be made to absorb more: hence 38 (rather 18) parts of sulphur, are substituted for 25 of oxygen. A great quantity of sulphurous acid gas was emitted.

The presence of oxygen in blende was inferred by Morveau, from its being transparent; yet the sulphurets of quicksilver, and of arsenic, are sometimes found transparent; the latter may be heated to any degree, without yielding sulphurous acid gas, or losing its transparency.

It is strange that metallic zinc cannot be directly united with sulphur.

Sulphuret of zinc is frequently found united with oxides, or sulphurets of other metals: some are absurd enough to form these varieties into separate species.

Some black blendes are coloured by red oxide of iron, but they become red when pulverized; when they are analyzed by

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muriatic acid, the sulphuretted hydrogen that is formed reduces the oxide to that ad minimum, from whence a mistake might arise. Some contain oxide, or sulphuret of lead; if they are treated with muriatic acid, the lead is taken up into the solution, but if the muriatic (rather nitric) acid is used, the lead remains in the residuum, as the sulphur is acidified, and throws down the lead in the form of a sulphate.

Sulphuretted hydrogen throws down a yellowish white hydro-sulphuret of zinc, from solutions of that metal; but it is necessary at last to saturate the growing excess of acid with a little potash. Alkaline hydro-sulphurets produce the same precipitation. This hydro-sulphuret of zinc is strongly acted upon by nitric acid. Muriatic acid disengages, without heat, much sulphuretted hydrogen from it. By heat alone, this hydro-sulphuret yields water and sulphurous acid, and is also changed into simple sulphuret or blende. Both natural and artificial sulphuret of zinc yield sulphuretted hydrogen, which is not contained in them, but produced from the water present.

This memoir is accompanied with a few notes, signed *A. G.* mine engineer, who observes, that Proust ought to have determined whether the native sulphuret of zinc contained metal and sulphur in the proportion indicated by his experiment, or in any other. The experiments of Bergman were made on very compound blendes, and are otherwise imperfect. A very pure yellow blende yielded 62 per cent. of zinc, 34 of sulphur, 1.5 of oxide of iron, 2.5 being unaccounted for, although neither water nor sulphuretted hydrogen appeared on distillation. Therefore in this blende 100 of zinc was united with 54.8 of sulphur, instead of 38 [should be 18] as stated by Proust, in the artificial sulphuret.

Native blende is infusible. Bergman thought iron was a necessary intermedium in the direct union of metallic zinc with sulphur. Morveau says that Dehne fused them together by merely covering the mixture with powdered charcoal.

Observations.—This paper is most incorrectly printed, as may be seen in some of our corrections. It must be observed that the production of 118 gr. of sulphuret from 125 of oxide is stated in words at length, so that the assertion of 38 parts of sulphur being substituted for 25 of oxygen is evidently an error, although it has been passed over, and even repeated by the annotator himself.

It is probable there may be two sulphurets, that ad minimum, containing 84.75 per cent. of zinc and 15.25 of sulphur; and that ad maximum, containing 64.6 per cent. of zinc and 35.4 of sulphur.

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RETROSPECT

OF

PHILOSOPHICAL, MECHANICAL,

CHEMICAL AND AGRICULTURAL

DISCOVERIES.

No. XX.] *July, August, September.* [1809.

AGRICULTURE.

Essay on the Question, "What is the best Rotation of Crops for light Land (Wheat being one) in a late and rather a moist Climate?" By A. M.—*Farmer's Mag. No.* 39.

In treating this question the writer remarks, that two objects ought always to be kept in view: first, that in cultivating the ground there may be obtained from it the greatest quantity of the most valuable produce; and the other, that this produce be obtained in such a way as least to exhaust the soil. The alternation of white and green crops is asserted to be the basis of every judicious rotation, and to point directly to both these principles; and the important object of agriculture is conceived to be the application of them to existing cases, and to provide against exigencies which may be overlooked in theory.

The rotation recommended for the land which is the subject of this Essay, is a four-shift course: 1. Turnips; 2. Barley or oats; 3. Grass; 4. Wheat. This short and simple course of cropping is considered to be complete for obtaining the best returns in the given circumstances, and may be immediately adopted on lands already in tillage. For as the thorough cleaning of the land from weeds is admitted to be the foundation of all good husbandry, the culture of a turnip crop on all dry soils is of infinite consequence in this respect; as, independent of its own particular value, its beneficial effects extend through the whole rotation; the land being free from weeds when the barley is sown; and indeed the complete tilth and mellowness of the ground, so properly aimed at in the preparation for this crop, not being to be procured in a manner equally advantageous in any other way; as the operations

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attending the culture of this valuable root, reduce the soil to a degree of pulverization which greatly assists the growth of both barley and oats, and is indispensable in the view of sowing down with grass-seeds, which is the crop next to follow; and the early period of the season at which the grass-seeds may be mown and cleared away, leaves ample time for finishing the sowing of the wheat at such a period, as to admit of its perfect growth and ripening. The objection, that the wheat crop is thrown too far back from the manuring, which must necessarily be confined to the preparation for turnips, is allowed to be not without its weight; but it is remarked, that the wheat, though not immediately succeeding the turnips, is placed at no very great distance, and that it could not be placed in immediate succession, as the turnips would not be eaten off sufficiently early to allow of the wheat being sown at the proper season; and it is contended, that from the short time which the grass is permitted to remain on the ground, there is no danger of the wheat being injured by the production of vermin; which too often abound upon an old clover ley; and the writer believes that it would be difficult, either by disposing the crops already mentioned in a different order, or by introducing others in addition to them, to form any arrangement which would on the whole be more advantageous; for if turnips are to enter at all into the system, they cannot be removed in time for an early sowing of wheat; and beans and other green crops are not suitable for the soil; beans being rather adapted for heavy lands; and peas, besides being otherwise precarious, of little avail for the amelioration of the ground; and besides, the uses of turnips are important in the feeding of cattle, and the consequent increase and improvement of manure.

The assumed theory of the rotation being supported by this kind of reasoning, notice is taken of various objections which may be offered; and these are candidly discussed and obviated; and where a three-shift course is preferred, a rotation of crops on the same principle is pointed out; and the several items of expenditure and income attending the courses recommended are given in appropriate calculations.

Observations.—This Essay is written with remarkable clearness of deduction, and couched in language which is but rarely found in the periodical publications on Agriculture: it merits the attention of every reader whose object is information, and will be found particularly interesting to those who cultivate the soil to which it more immediately relates.

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On Peat Ashes. By a Friend.—Farmer's Mag. No. 39.

THE writer states, that at Flitwick in Bedfordshire peat-ashes are sold for manure, and used as a top-dressing for barley, at the rate of from forty to sixty bushels on an acre: they are spread on the lands in March, after the seed is sown; and sometimes also on clover; they are also admirably useful as a manure for turnips, and are best distributed by means of a drill. The small pieces of peat which break from the spade are likewise used, without burning, and with much advantage.

When the peat is to be burnt, after the quantity required has been got together, a portion of it, sufficient to kindle a large heap, is dried, of which a conical pile is then made and fired; and as soon as the flame or smoke penetrates through any of the crevices, it is kept back by fresh peat, just dry enough to be free from water; and the pile is kept up in this manner till the whole is reduced to ashes; and the slower the process the better.

Observations.—This mode of burning peat is well described, but is not new (as the writer imagines), having been long practised in the northern parts of England, where peat-bogs are much more numerous and extensive than in the midland counties.

On the Management of Harvest Work. By ARATOR.—*Farmer's Mag. No. 39.*

THOUGH this paper contains but few new observations, yet it affords some hints not altogether undeserving of attention. The writer advises not to let *wheat* on any account stand till it is quite ripe, because, independent of the hazard from winds, much may be lost in carrying it to the stack-yard: small sheaves are asserted to be best, because they are sooner ready for the cart, and more equally dried; and on these accounts it is said to be more advisable to direct the reapers to tie them up with one length of straw instead of two, as is usually done. The value of a good builder is particularly commented on, and he is directed so to slope the sheaves as that the rain which comes against the outside of the stack may run off, instead of getting into the centre. The harvesting of *barley* well is considered to require much more management than the harvesting of either wheat or oats, as it is altogether unfit for stacking, except in a very dry state; and when grass-seeds are grown with it the difficulty is increased: it is recommended not to cut it wet, if it can possibly be avoided by delay; and, when cut and become dry, to tie it in sheaves, which

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will much facilitate the carting it. The directions for harvesting *oats* vary so little from those which relate to barley, as not to merit a distinct notice; but it is said to be advantageous in stacking both these kinds of corn, to have a funnel from the open side, and to carry an opening up the middle of the stack from nearly the bottom to the top, by which means the foul air will be thrown off, which may be generated by the heating of the corn. *Beans* and *peas* are directed to be as ripe as possible before they are cut; and when the stalks are entirely dry, to be tied up in sheaves, and not to be carried to the stack-yard till they be free from wetness of every kind. It is suggested that these grains should seldom be built into very large stacks, except in particularly dry seasons. Early and effectual *covering*, for all sorts of corn, is pointed out as an object of the first importance; and the caution of *carefully gathering*, even at a considerable increase of expense, is strongly inculcated.

Observations.—The most common subjects may be occasionally treated in such a manner, as to afford some useful hints to those who think themselves the best informed. Of this nature is the paper under consideration; which, if it does not convey information, reminds the agricultural reader of a very important branch of his duties.

Successful Experiment of a Merino-Shetland Cross Sheep-shearing. By Mr. THOMPSON, *of Red Hill Lodge.—Agric. Mag. No.* 24.

IT is stated that the Shetland is a diminutive sheep, carrying a very light fleece, which consists of fine wool intermixed with a considerable proportion of coarse hair. The celebrated Mr. Tollet was induced, from a knowledge of the propensity of the Merino ram to expel kemps, and to increase the quantity of wool, to procure three Shetland ewes to cross with the Merino breed. The weight of the fleeces of three wedders from this cross was, *thirteen pounds four ounces*; while the fleeces of the three Shetland mothers weighed only *two pounds fourteen ounces*; and the improvement in the quality is stated to be almost beyond conception. Taking the value only at double, and reckoning the Shetland wool at one shilling a pound, the produce of each ewe was short of a shilling; while that of her son by the Merino ram was nearly nine shillings!

Observations.—This is believed to be the first account of an experiment made by crossing the Shetland with the Merino race of sheep; and the result is such as might have been expected from the penetrating mind of Mr. Tollet. As an authentic account of a new experiment, this paper is unusually interesting.

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Comparative Experiment on feeding a Berkshire Pig, and one of the improved Essex. By C. C.—Agric. Mag. No. 24.

THIS experiment was made by a farmer, who could neither be prevailed on to exhibit his pigs at the Smithfield Christmas show, nor to communicate his proceedings to the public; but the writer *wormed* out of him the particulars, and sent them to the Magazine. He states that the sows which produced both the pigs farrowed in January, within a few days one of the other; the Berkshire sow was of the middle size, but of the true breed, and was purchased in pig at Smithfield: the Essex sow was bought young, and sent to a boar of the same race. After the pigs had recovered from cutting, the farmer selected two barrow pigs, which appeared the heads of both the litters; this was at the beginning of April, and they ran picking about the yards with the rest till clover-time: the Essex pig with his keep looked already fat, compared with the Berkshire, which, though thin, grew wonderfully in size; when they came to the clover the Berkshire gained flesh very fast, but the Essex after a while seemed disagreeably affected by the clover, and lost ground: in the stubbles the Essex pig made himself absolutely fat, and the Berkshire was grown very large.

They were both put up to fatten, each in a separate sty, on the 20th of October, and their food was barley and oats ground together, and after Christmas a small portion of ground peas was also added; and they had pot-liquor and common wash, and now and then a little skimmed milk. The experiment was conducted on this singular plan, and steadily executed throughout. The farmer resolved that the large pig should have only exactly so much, and no more, meal by measure, than the small one chose to eat, and he himself, during the whole fourteen weeks, two or three feedings excepted, measured the feeds; the result was, that sometimes one pig left his allowance, and sometimes the other, but the quantity of food consumed by each was precisely the same. They were killed on the 1st of February; the Berkshire pig weighed 40 stones, 2 lbs. (8 lbs. to the stone); the Essex pig 31 stones, 7 lbs.: the Essex pig was most beautifully and thoroughly fattened; the Berkshire pig also died very handsome and very fat, but not to be compared with the other. They were both pickled down for use, and were equally good eating. It appears from the account that the Berkshire pig exceeded the Essex by 8 stones, 3 lbs.

Observations.—This experiment appears to have been conducted upon a new principle, and with an exactness seldom to be met with. The superiority of the Berkshire breed over the Essex seems to be well substantiated; and it is much to be wished that the comparative value of other breeds of pigs may be experimentally ascertained in the same manner.

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On Merino and New Leicester Sheep, in Answer to Mr. Hunt. By JOHN WRIGHT, *of Pickworth, near Stamford.—Agric. Mag. No.* 25.

On the Fallacy of experimental Evidence, and the Influence of Misrepresentation, in Answer to Mr. Wright. By JOHN HUNT, *of Loughborough.—Agric. Mag. No.* 26.

THESE papers are a continuation of that tedious and uninteresting controversy, of which we have been compelled to notice too much already. We had hoped that the matter would have been left at rest, till the termination of the experiment, instituted for the decision of the question; as noticed in our last number, page 215.

On Threshing Machines. By WILLIAM SHEPHERD, *of Woburn, Bedfordshire.—Agric. Mag. No.* 25.

THIS short communication on threshing machines ought to be read with the recollection that the author is a manufacturer of those implements. A machine described in a former Magazine is much censured, and a challenge is thrown out, for either a single bet, or a sweepstakes, that Mr. Shepherd will himself turn out an implement of this kind superior to one made by any competitor professing to erect such machines.

Observations.—As we are not working mechanics, we dare not accept the challenge of the Woburn plough-wright; and as we are *poor* scholars, we must not presume to contend with the *argumentum ad crumenam*, but we had always understood that a wager was never resorted to in controversy till all other arguments had been defeated.

On a dangerous Practice in bleeding Horses, and the proper Method of performing that Operation. By CLERICUS.—*Agric. Mag. No.* 25.

THE practice censured is the usual one of common blacksmiths, of opening the vein with a fleam and bloodstick, and suffering the blood to flow at random on the ground; by which means it becomes impossible to ascertain the quantity taken away. The use of the lancet is recommended with many appropriate observations.

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On Dairy Cows, with Inquiries respecting the Alderney Breeds. By a SURREY FREEHOLDER.—*Agric. Mag. No. 25.*

THE writer, who cultivates a small quantity of land for the purpose of deriving some advantage from it in these times of excessive taxation, considers the dairy a material object in this point of view, and consequently that it is of importance to select the useful cows for this purpose. He observes that the best kind of large cows not only require a considerable sum to purchase them, but consume much food, and he considers it more economical to keep small or middling bred cows, which he believes will yield a greater proportionate return for the food they consume, than those of the larger breeds: and he apprehends the Alderney best adapted for the dairy on a small scale; but, having no facts to offer, concludes with requesting information on the subject.

Observations.—This unimportant paper claims no attention, except from those who have no other avocation than answering the most simple questions through the medium of a Magazine.

On Turnip and Potatoe Cultures, with miscellaneous Remarks. By C. M.—*Agric. Mag. No. 25.*

REMARKS on an essay on the culture of potatoes and turnips, by Dr. Campbell, of Lancaster, which was published three years since in the Communications to the Board of Agriculture. The article excited no interest at the time of its publication, and will deservedly sink into oblivion, notwithstanding the attempts of this writer to drag it back into notice.

On Draught Oxen. By a PATRIOT.—*Agric. Mag. No. 25.*

A FEW trivial observations on the use of oxen, from having observed their steadiness and docility in the carts of the Earl of Mansfield, which pass between Caen Wood (called *Kenwood* in the paper) and London. The article is addressed to those who apprehend a defect of speed and utility in bullocks for the carting business, which apprehension is stated to be entirely without foundation.

Observations.—We conceive that the *patriotic feelings* of the writer obtained that insertion for his communication, which it was clearly not entitled to from any internal merit.

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On the Cultivation of Lucerne. *By* NOVISSIMUS.—*Agric. Mag. No.* 26.

THIS writer states, that he has been many years a cultivator of lucerne. He conceives it not good counsel to confine this plant to rich soils, since almost any dry soil will answer remarkably well; but the grand error appears to him to be the sowing it broad-cast, since this precludes the opportunities of freeing it from weeds, which are afforded by the drill husbandry.

Observations.—These few observations are entitled to notice as founded on actual experience, and as they tend to correct some mistaken opinions which have been given to the world in Mr. Arthur Young's various agricultural publications.

An Account of planting Oaks on the Estate of the Earl of Mansfield. *By* WILLIAM BEATTIE, *of Scone, near Perth.*—*Trans. Soc. Arts, Vol.* 26.

THE number of oaks planted at Scone, by the direction of the Earl of Mansfield, was ninety-six thousand, part of which were raised from acorns and part from young plants; the ground planted, consisted of steep banks on the side of a brook, and part of it was common moor land. The sloping banks were planted in the usual way, but on the moor land the furze, which grew there, was burnt, and the land ploughed as well as the nature of it would admit, and before planting laid up in ridges of four or five feet breadth, which, with some open drains cut through them, laid the whole completely dry; and the oaks were planted on the tops of the ridges.

This is represented to be a new mode of treating plantations, and to have been suggested by the Noble Earl; and it is said to be universally approved of, inasmuch as it lays the land dry, and prevents the young plants from being thrown out by the frost in winter, and at the same time places a greater quantity of surface mould round them, in which they grow freely while young, and thrive well, though the substratum of the soil is very indifferent. The truth of this statement, as it obtained the premium of the Society, is supported by the most respectable certificates.

Observations.—The Earl of Mansfield has certainly promoted the public interest by planting such a number of oak trees, which may contribute at a future period to the service of that bulwark of the British empire, the royal navy; and the method of plant-

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ing them on four or five feet ridges is very safe and judicious, particularly on wet soils; but it is by no means a *new* method, as the writer supposes; for it has long been in use in the midland counties of England, with respect to all kinds of timber; and not only oak, but also ash and elm, have been planted in this manner on the estates of Earl Spencer and the late Dr. Radcliffe, by the lately-deceased steward, as long as thirty years since; and the flourishing state of those plantations fully justifies Lord Mansfield in suggesting the utility of the plan in forming plantations on the moor-lands of Scotland. The instances mentioned are by no means the only cases we could adduce of the success of this method of planting, but they are believed to be some of the earliest experiments; and indeed the plan is now universally adopted in the midland counties for plantations of ash-poles and osiers, and of all kinds of underwood, which is not designed for hedge-rows.

An Account of Land gained from the Sea by Embanking. By William Lawrence, *Esq. of Maldon, in Essex.—Trans. Soc. Arts, Vol. 26.*

The land which is the subject of this paper, is the property of the dean and chapter of Saint Paul's, and is held by Mr. Lawrence on a lease for 21 years, renewable in the usual mode of church leases. The lease was refused by the tenant of the adjoining lands, because he did not think the land reclaimable, and was then granted to the present applicant. The embankment is 290 rods in length, twenty feet wide at the base, and four at the top, and five feet high; the slope on the side next the sea being ten feet; and on the land side, six feet: the embankment is made of soil only, and there are two sluices, or gutters, under it, which drain the land inclosed: the quantity of ground inclosed was one hundred acres within a few poles, and the expense of the embankment, including tools and extra expenses, amounted to 507*l.* 11*s.* besides what was paid for levelling the hills and holes, and in making ditches, gates, &c. In making the embankment, the soil was not thrown loosely together, but was composed of the earth taken from the ditch on the land side, and the sward and soil from off the foreland on the sea side, which were thrown together to form the wall, and after every layer well rammed or pounded together by the workmen. No grass seeds were sown on the bank when finished, because it was so much impregnated with salt, that it might have been scorched up by the heat of the summer, but it is intended to sow it with seeds the second year, when a tolerable turf may be expected, at least for sheep feed. Part of the land was broken up in the winter, and sown with black oats by way of experiment; but as this commu-

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nication was made to the Society in the month of April, the produce was not then known.

Observations.—The reclaiming land from the sea has been pursued with so much advantage both to the reclaimers and the country at large, that we are glad to see the mode of accomplishing so desirable an object so extensively communicated, as it must be by the publication of the methods adopted in the Transactions of the Society of Arts, and in the Communications to the Board of Agriculture. The benefits of the practice are too plain to be overlooked, when the means of accomplishing it are well understood.

CHEMISTRY AND MINERALOGY.

On the acetous Fermentation; and on the Manufacture of Vinegar. By C. A. CADET.—*Ann. de Chim. No.* 186.

CHEMISTS have not yet shewn which of the proximate principles of vegetables are the most apt for acetification; nor in what proportion these principles enter into the composition of vinegar. All vegetable fermentescible substances yield, it is true, acetous acid; but its strength or quantity cannot be estimated beforehand. Experiments of this kind are easily made, as the acetous fermentation takes place more readily in small masses than in large ones.

Notable differences exist between the vinegar made from different substances; but wine vinegar has usually a specific gravity of 1.0135 to 1.0251; it not only contains acetous acid, but also the sulphates of potash and lime (which are probably merely accidental ingredients), extractive matter, and the tartareous, citric, malic, and oxalic acids. The three latter acids are however frequently wanting, even in very good vinegar. As the most saccharine must yields the best wine, so the best wine, as those of Languedoc, Spain, and Italy, yields the strongest vinegar, although too dear for common use.

It is well known that either wine or vinegar may be made from sugar; for the English colonists prepare a wine from 250 lbs. of sugar (treacle), 2 muids (barrels) of water, and 4 lbs. of yeast; this wine is coloured with litmus, and scented with some essential oil. Fabroni has made wine from 864 lbs. of sugar, 24 of gum arabic, 24 of tartar, 3 of tartarous acid, 36 of gluten of wheat, and 1728 quarts of water. Parmentier has made a good muscadine wine from 216 lbs. of sugar, 9 of crystals of tartar, 79 of elder-flowers, and 307 of water. It is however of great con-

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sequence to know how much vinegar a given quantity of sugar would yield, and what proportion of water and ferment ought to be added.

A solution of sugar-candy did not ferment until one fifteenth of its weight of yeast was added, and then it fermented readily in the temperature of 12 to 20° (68° Fahr.). Mucoso-saccharine matter (treacle) ferments of itself, provided it is diluted in a sufficient quantity of water. Solution of gum began to become acid very quickly, and its fermentation was promoted by yeast. Extractive matter does not ferment, but becomes putrid. Yeast produces no effect, not even upon extract of liquorice, which seems to contain a saccharine principle. Starch does not ferment by itself; but flour ferments, with difficulty indeed, and after three or four days the liquid begins to putrefy.

Two hundred and eighteen grammes (7 oz.) of flour boiled in 1713.25 gr. (56 oz.) of water, assisted by 15 gr. (½ oz.) of ferment, kept working for twenty-four days, and produced as much vinegar as saturated 34 gr. of potash.

The same quantity of starch boiled with a similar proportion of water and ferment kept working for thirty-five days, and produced as much vinegar as saturated 42.3 gr. of potash.

Although sugar, assisted by yeast, passes from the alkoholic to the acetous fermentation, yet the mixture of alkohol with water does not ferment, unless extractive matter, mucilage, or boiled starch is added, and the alkohol is not too predominant in the mixture.

A. 306 grammes (10 oz.) of sugar, 2142 gr. (70 oz.) of water, and 20.22 gr. (5 drachms, 24 grains French) of ferment, began to work the next day; the fermentation lasted twelve days. It required 6 gr. of potash to saturate 122.5 gr. (4 oz.) of this vinegar, marking 6° hydr. Eight ounces of the vinegar yielded 2 drachms of crystallized sugar.

B. 153 grammes (5 oz.) of sugar, 153 gr. of alkohol, 2142 gr. of water, and 20.22 gr. of ferment, began to work the same day; the fermentation lasted 12 days. It required 4 gr. of potash to saturate 122.5 gr. of this vinegar, 1° hydr. Eight oz. (245 gr.) of the vinegar yielded 2.06 gr. of sugar; it tasted very weak, and yielded alkohol on distillation, with some traces of acetic ether.

C. 459 grammes (15 oz.) of sugar, 2142 gr. of water, and 20.22 of ferment, began to work the same day; the fermentation lasted 23 days. It required 8 gr. of potash to saturate 122.5 gr. of this vinegar, 8° hydr. Eight oz. of the vinegar yielded nearly an ounce of sugar.

D. 306 grammes of sugar, 153 gr. of alkohol, 2142 gr. of water, and 20.22 gr. of ferment, began to work the second day; the fermentation lasted eight days; carbonic acid gas was emitted,

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but the liquor did not redden syrup of violets. It yielded 38 gr. of weak alkohol per quart on distillation.

E. 245 grammes (8 oz.) of sugar, 61 gr. (2 oz.) of gum, 2142 gr. of water, and 20.22 gr. of ferment, began to work the first day; the fermentation lasted fifteen days. It required 8 gr. of potash to saturate 122.5 gr. of this vinegar, 8° hydr. The vinegar was very strong, and alkohol being poured into it, a month afterwards, precipitated near 30.5 gr. (1 oz.) of mucilage.

F. 306 grammes of sugar, 122.5 gr. of mucilage, 2142 gr. of water, and 20.22 gr. of ferment, began to work the first day. It required 8 gr. of potash to saturate 122.5 gr. of this vinegar, 5° hydr. Alkohol threw down more mucilage than from the preceding vinegar.

G. 459 grammes of sugar, 2142 gr. of water, and 38 gr. (1 oz. 2 dr.) of ferment, began to work the first day; the fermentation lasted ten days. It required 8 gr. of potash to saturate 122.5 gr. of this vinegar, 6° hydr. 122.5 gr. of this vinegar yielded 8 gr. of sugar, which had not fermented.

From these experiments it may be concluded: 1. That the alkohol added to the fermenting liquor, although it be partly changed into vinegar, cannot be substituted for sugar; 2. That when the sugar is in too great quantity, part of it does not ferment; 3. That mucilage hastens the fermentation, but, like sugar, the superabundant portion remains unaltered; and, 4. That too large a proportion of alkohol prevents fermentation altogether.

Many experiments were made to determine the best proportion of ingredients to form vinegar; the following was the best.

H. 245 grammes of sugar, 1719.5 gr. of water, and 1575 gr. (4 dr. 15 grains French) of ferment, began to work the first day; the fermentation lasted twelve days. It required 12 gr. of potash to saturate 122.5 gr. of this vinegar, 5° hydr. The vinegar was very strong, agreeably tasted, and did not contain any sugar.

I. By diminishing the quantity of water some of the sugar remains undecomposed; thus: 218 gr. (7 oz. 1 dr.) of sugar, 857 gr. (28 oz.) of water, and 15 gr. (4 dr.) of ferment, did not begin to work till two days afterwards; the fermentation lasted 21 days, being very slow. It required only 8 gr. of potash to saturate 122.5 gr. of this vinegar, 6° hydr. The liquor was still very saccharine, and tasted like simple oxymel.

K. The following proportions appeared most likely to cause alkohol to be totally converted into vinegar. 108.5 grammes (3 oz. 4 dr. 36 grains French) of sugar, the same quantity of alkohol, 857 gr. of water, and 15 gr. of ferment, began to work two days afterwards; the fermentation lasted seventeen days. It required 6.6 decigrammes of potash to saturate 122.5 gr. of this vinegar, 4° hydr. The liquor had a vinous smell, and half the alkohol employed was separated by distillation.

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Sugar therefore is the essential ingredient in the formation of vinegar; mucilage is partly changed into vinegar, and accelerates the fermentation, but disposes the liquor to putrefaction, unless prevented by adding alkohol. This last is imperfectly changed into vinegar, but it renders the vinegar sharp, makes it keep well, and when used for infusing aromatic plants, takes up the resinous odoriferous particles.

Mr. Hebert of Berlin pretends to have made vinegar in two months, from four parts of rectified malt spirits, and seventy-two of water; but this experiment did not succeed in France. The vinegar commonly sold contains alkohol, and the first portion that comes over in distillation is alkoholic. Mr. Lowitz has separated alkohol from vinegar by freezing, and subsequent distillation from powder of charcoal.

The quantity of potash required to saturate a given quantity of vinegar indicates the quantity of acid which it contains; and the quantity of acid appears to depend upon the quantity of sugar originally contained in the fermented mass.

Hence taking the experiment H. for the basis of the calculation, it appears that the sugar employed in the formation of acetous acid is to the potash necessary to saturate it, as 1.2668 to 1.0000; from whence there may be calculated the quantity of sugar that ought to be added to any weak vinegar in order to bring it to any required strength. The same experiment also shews that 100 parts of good vinegar are formed from 12.410 of sugar, 0.799 of yeast, and 86.791 of water; whence the proportion of yeast to be added may be deduced; and on this foundation an estimate may be made of the quantity of saccharine matter contained in a vegetable, when it is so enveloped in other principles as not to be separable by the ordinary methods. This may be performed by making a decoction of the plant, fermenting the decoction, and assaying the acid with potash. If the decoction reddens litmus, the malic, citric, oxalic, or tartareous acid contained in it must be saturated with lime, and the liquor filtrated before fermentation.

L. 218 grammes of honey, 1713.5 gr. of water, and 15 gr. of ferment, began to work the sixth day; the fermentation lasted seventeen days. 4.2 gr. of potash were required to saturate 122.5 gr. of the vinegar, which exhibited 5° hydr.

M. The same quantity of manna, water, and ferment, began to work the next day; the fermentation lasted 15 days. 1.1 gr. of potash were required to saturate 122.5 of the vinegar, 6° hydr.

N. 489.5 grammes (16 oz.) of figs, 1713.5 gr. of water, and 15 gr. of ferment, began to work the next day; the fermentation lasted fifteen days. 5.3 gr. of potash were required to saturate 122.5 gr. of the vinegar, 5° hydr.

O. The same quantities of raisins, water, and ferment, began to work the next day; the fermentation lasted fourteen days. 4.

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gr. of potash were required to saturate 122.5 gr. of the vinegar, 5° hydr.

Hence honey appears to contain 38.78 per cent. of sugar; manna, 10.1653; figs, 24.8351; and raisins, 10.7453 (or rather, 18.7453).

If soluble tartarite of potash is added to the fermenting mass, the vinegar is made bitter, but not increased in strength. If acidulous tartarite of potash is added, only one sixtieth is taken up, and the vinegar becomes rather stronger, on account of the excess of tartarous acid. The spontaneous decomposition of vinegar is not owing to the tartar it contains, but to mucilage and vegetable albumen.

Neither albumine nor gelatine accelerates fermentation.

An ounce (30.5 grammes) of the best Orleans vinegar takes up 3 gr. of potash; common Paris vinegar only takes up 2.4 gr.; radical vinegar takes up 42 gr. Some persons sell very strong vinegar, which is supposed to contain a mineral acid, but it contains only alkohol, and a considerable portion of acetic acid, and may be formed by adding 34.7 gr. of acetic acid, and 16 gr. of alkohol, to 1000 gr. of Orleans vinegar.

It is most probable that radical vinegar contains less carbone and more oxygen than acetous acid. If they differed only in concentration, acetic acid should be formed by fermentation, on augmenting the acidifiable principles, which is not the case; neither can common vinegar be changed into acetic acid by abstracting the water, either by freezing, by combination with a base and subsequent separation, by distillation upon muriate of lime, or other processes.

A small proportion only of acetous acid can be formed in a given quantity of water, because, when evolved to a certain degree, it prevents the further continuance of the decomposition of the vegetable substance.

Acetic acid can only be procured from the acetites of those metals which can be partially reduced by taking carbone from the vinegar.

94.5 grammes of radical vinegar saturated with caustic potash, yielded by distillation 11.5 gr. of water, and 133 gr. of a saline mass. The same quantity of good distilled vinegar yielded 87.5 gr. of water, and 15 gr. of salt. As the radical vinegar became solid before it was saturated with potash, 15 gr. of water were obliged to be added to keep it fluid; this however was deducted from the product as mentioned above. These are not the absolute quantities of water in the two acids, but are merely relative.

Vinegar may be made in all places by properly adjusting the quantities of saccharine matter, water, and ferment. Excellent vinegar has been made by Messrs. Blanche and Favier from cabbages, with some brandy and sumach. Alkohol may be employed in lieu of some of the sugar, provided it does not exceed the fourth

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part of the sugar. But the ridiculous custom of throwing a dog or dead cat into the working tons ought to be prohibited.

The quantity of sugar that the substance contains ought to be previously ascertained by fermenting 8 oz. of it with 56 oz. of water, and half an oz. of yeast, or baker's leaven, which latter is preferable, because yeast sometimes gives a disagreeable taste to the vinegar. The mixture is to be put into a vessel, one half of which is to be left empty, and kept in a temperature of 20 deg. (68° Fahr.), for 25 or 30 days, being stirred morning and evening. The strength of the vinegar thus obtained, being ascertained by adding potash, it will be easy to calculate how much saccharine matter must be added to form vinegar of the required strength.

In large manufactories, it is best to employ small barrels, placed as in saltpetre-houses, in order that their contents may be easily drawn off; and no brass cocks nor pewter cranes should be used. The barrels should be placed in a stove, so that all may receive an equal degree of heat regulated by thermometers placed in different parts of the building. The fermentation being finished, the vinegar should be filtered through sand, previously washed with muriatic acid.

Observations.—The experiments here mentioned would be of considerably more value if it were not for the extremely absurd manner in which they are related. In some both the old and the new weights are given, others have only one of these metrical systems, and in others again, the old weights are used in one part of the experiment, and the new in another, perhaps even in the very same sentence.

The real substances employed can in general only be guessed at, as the generic names of the new nomenclature are used, such as sugar, alkohol, ferment, without those restrictive adjections which are necessary to identify their designation.

On the Action of nitric Acid upon Cork. By Mr. Chevreul.—*Ann. de Chim. No.* 186.

Brugnatelli and Bouillon La Grange have described the action of nitric acid upon cork, and the properties of the acid thus obtained; but many persons retain doubts concerning it.

Six parts of nitric acid at 29 hydr. were cohobated upon one of cork. The cork became yellow, emitted nitrous gas and carbonic acid gas, and a considerable quantity of prussic acid was formed. When the action of the acid was weakened, the whole was evaporated to an extract, and then heated in water for some hours; when the water cooled, a matter like wax fixed upon the surface

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of the liquor, and large flakes of another matter were collected at the bottom.

These flakes were insipid, insoluble in water or alkohol; they were white, but grew rather brown in the air. Nitric acid at 32° hydr. did not act upon them. They burnt upon a live coal, puffing up, and exhaling a smell of empyreumatic vinegar. The charcoal was bulky, and rather hard. They were, therefore, the woody part of the cork.

The wax-like matter had very little taste. It was insoluble in water; boiling alkohol dissolved it; and deposited, on cooling, a white substance like wax; water added to the supernatant solution, threw down a straw-yellow resinous substance, which turned reddish in the air, and remained acid in spite of repeated washings. It yielded by distillation a concrete oil, and an acid which precipitated acetate of lead. The solution to which water had been added, became yellow on evaporation, and acquired a smell of bitter almonds, so that it held some yellow matter and probably some prussic acid.

The liquid in which the flakes were originally deposited, was yellow, acid, and bitter; it precipitated lime-water and calcareous salts; it rendered solution of indigo green; that it contained some iron was shewn by nut-galls; but when the excess of acid was saturated, it did not precipitate gelatine. It exhaled the smell of vinegar, but it yielded nitric acid on distillation. On evaporation it deposited suberic acid mixed with some yellow matter, and afterwards yielded crystals of oxalic acid. The mother water was bitter, and contained yellow matter, from which the remaining oxalic acid was separated by adding lime-water in excess, then caustic potash to separate the lime, by which means small gold-colour crystals of yellow matter with potash were obtained. The mother water of these crystals, yielded in one trial benzoic acid by the addition of muriatic acid.

The suberic acid was purified from the yellow matter by washing with cold water, and successive solutions in boiling water, by which about 5 grammes of acid were procured from 60 gr. of cork. The acid is white, opake, and pulverulent. It tastes sour without any bitterness. Light does not alter its colour. It requires 38 parts of water at 60° (140 Fahr.), and 80 parts at 13° (55 Fahr.), to dissolve 1 of the acids. It is totally volatilized upon a live coal, yielding a smell like tallow. It melts by a slight heat, and if spread on the sides of the vessels it crystallizes in needles on cooling. It also sublimes unaltered into white needles, leaving some traces of charcoal.

Liquid suberic acid reddens litmus; it does not precipitate the aqueous solutions of lime, strontian, and barytes, nor their saline combinations. The precipitate obtained by Brugnatelli from lime-water, and calcareous salts, probably originated from a mixture of oxalic acid; and indeed his acid seems also to have

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contained bitter matter, and some resinous substance. When lime-water saturated with suberic acid was evaporated, the calcareous suberate fell down in white flakes: muriatic acid decomposed this salt, and the muriate of lime being separated by washing with warm water, the suberic acid was left as a very white powder. The same depuration of the acid from colouring matter was effected by using barytes instead of lime. The suberates of ammonia and of the fixed alkalies are decomposed by the sulphuric, muriatic, and other acids.

Suberate of ammonia precipitates solution of alum, as also the nitrate and muriate of lime, if the calcareous solutions are concentrated. It also decomposes almost all metallic solutions.

Copper is thrown down of a pale blue; cobalt, rose colour; zinc, white; &c.

Suberic acid precipitates neutral solution of silver, white; muriate of tin ad minimum, white; sulphate of iron ad minimum, white; nitrate and acetate of lead, white; nitrate of quicksilver, white. It does not precipitate sulphate of copper, or of zinc.

La Grange says, that the suberic acid decomposes nitrate of quicksilver, and the sulphates of copper, iron, and zinc; and in another place, that it yields quicksilver and zinc to the three mineral acids, and iron and copper to the sulphur: this seems to imply a contradiction.

Suberic acid was dissolved in 12 times its weight of nitric acid at 32° hydr.: the solution being boiled, no alteration took place, for suberic acid was deposited after the liquid had been cooled for some hours: the addition of water promoted this separation; no crystals of the acid could be obtained by this mode of procedure.

Suberic acid is soluble in alkohol, and partly separable from it by water.

This acid does not render sulphuric solution of indigo green, as stated by La Grange; this colour is produced from the yellow bitter matter in his acid mixed with the blue of the indigo. The same impure acid renders solution of copper green, but the white purified acid does not.

Suberic acid has a great analogy to the sebacic acid of Thenard; but the latter assumes a crystalline form either in water or in alkohol.

On the Oxides of Copper. *By Prof.* Proust.—*Journal de Phys. July* 1807.

Red octaedral oxide of copper is the oxide ad minimum, and contains 84.75 per cent. of copper, and 15.25 of oxygen; so that 100 parts of copper have absorbed between 17 and 18 of oxygen.

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Black oxide contains 80 per cent. of copper, and 20 of oxygen; so that 100 of copper have absorbed 25 of oxygen.

Observations.—Mr. Proust still maintains his opinion of there being only two proportions in which oxygen and sulphur combine with metals, or in which similar combinations of two ingredients are made. These speculations certainly tend to hinder investigation, although they possess, in some degree, the appearance of truth.

Analysis of Cinnamon Stone. By Mr. LAMPADIUS.—*Journ. de Phys. July* 1807.

CINNAMON stone contains 42.8 per cent. of silica, 28.8 of zircone, 8.6 of alumine, 6 of potash, 3 of oxide of iron, 2.6 were lost in the previous calcination, and 4.4 in the subsequent operations.

Observations.—Werner has already placed cinnamon stone in the zircone genus along with hyacinth; but Klaproth has not been able to find any zircone in this stone, as will appear when we give his analysis.

On a fetid Variety of Fluor Spar. By Mr. ALLUARD *the Elder.* —*Journ. de Phys. August* 1807.

THE author having accidentally dropped a specimen of fluor spar which Mr. Watts had sent him from England, discovered that it exhaled a fetid smell when struck. The fluor was deep violet, lamellar, and very fetid by friction.

On acetic Ether. By Mr. THENARD.—*Ann. de Chimie, No.* 187.

IN the preparation of acetic ether, no gas is disengaged, and nothing but acetic ether is formed without even any water. By dissolving acetic ether in water, adding caustic potash to the solution, and distilling, the ether is changed into alkohol and acetic acid.

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On nitric Ether. By Mr. THENARD.—*Ann. de Chim. No.* 187.

ETHEREAL nitric gas produced in the preparation of nitric ether, being washed with water, then placed in contact with a solution of caustic potash, and frequently agitated, was decomposed in 36 hours. The solution contained alkohol, nitric acid, some nitrous acid, and a considerable proportion of acetic acid. The gas itself contained more nitrous gas than before.

A very dilute solution of caustic potash produces the same results, only it emits a large quantity of very ethereal gas, which contains more or less nitrous gas. A very concentrated solution is less effectual. Water alone produces the decomposition of ethereal nitric gas.

Nitric ether mixed with alkohol of potash, was left for some time in a bottle, filled nearly full and well stopped. Acetate of potash was immediately produced; and, in the course of a day, crystals of nitrate of potash were deposited.

Hence, by supposing that nitric ether will yield only nitrous acid, acetic acid, and alkohol; that no water is formed in the preparation of muriatic acid; and that acetic ether may be readily changed into alkohol and acetic ether; it will follow that muriatic, nitric, or acetic ethers are formed of those acids, or their principles combined with alkohol or its principles. Perhaps the acetic acid which may be produced from nitric ether, is already combined in it, with alkohol, so as to form an acetic ether.

On Kermes Mineral. By Mr. CLUZEL, *Junior.*—*Ann. de Chim. No.* 188.

THIS paper obtained the prize offered by the Pharmaceutical Society at Paris.

According to the usual process, 16 parts of sulphuret of antimony grossly powdered, and 4 of nitre fixed by charcoal, are boiled for two hours, and the solution filtered, and laid aside; 3 parts of fixed nitre, and 20 of water, are then added to the residuum, boiled, and the solution separated as before; after which the residuum is again treated with 2 parts of fixed nitre, and 20 of water. The kermes is deposited from the solutions, washed with cold water until it becomes insipid, and then dried by a gentle heat. But the kermes thus obtained, has not the purplish-brown colour, nor velvety appearance, which is so much desired by apothecaries. The residuum being examined, appeared to be

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merely sulphuret of antimony, and might of course be employed in the preparation of a fresh parcel of kermes.

In order, therefore, to exhaust the antimony, 120 grammes of antimony were pulverized and treated repeatedly with 30 gr. of American pearl-ashes, and 300 gr. of filtered water. Each time fresh pearl-ashes and water were added, the mixture boiled for half an hour, then filtered into a warm vessel, left to stand for 24 hours, again filtered, the kermes washed with cold water, and dried on a stove by a heat of 25° Cels. (or 77° Fahr.). The operation was repeated 95 times. Kermes was obtained in the first 91. The 92d yielded only a few atoms of it, and the three last none at all.

A single application of a sufficient quantity of alkali is able to perform what, in the old method, required so much labour; for on boiling 16 grammes of sulphuret of antimony with 360 gr. of American pearl-ashes, and 4000 gr. of filtered water, for half an hour, and treating the solution in the usual way, scarce any residuum was left.

All the preceding specimens of kermes were pale, and not velvety.

Having discovered the proportion of potash, the quantity of the water was varied; when 2000, or 1000 gr. were employed, the kermes was heavier, and duller, so as to resemble powdered bricks. Six, 8, and 10 thousand gr. yielded very pale kermes. Nevertheless, in manufacturing kermes in large quantities, even 4000 gr. of water would be too large; it being only requisite there should be a sufficient quantity of liquid for the kermes to be slowly deposited, and that no crystallization should take place, as otherwise the precipitate would lose some of its colour by the extreme minuteness of its particles.

The oxygen contained in water that has not been boiled, seems to be the cause of the inferior colour of kermes when unboiled water is used to wash the precipitate; for when water that had been boiled, and then cooled in a close vessel, was employed, the colour was much better; indeed it is probable that kermes might be rendered entirely white, by aërated water abstracting the hydrogen from it. In like manner, kermes deposited from a solution agitated in contact with air was much paler than usual; and again, a very pale rather yellowish kermes was obtained when the filtered solution was received in cold unboiled water, or even in boiling water. Part of this alteration of colour arises from a white precipitate that falls down as soon as the solution, holding kermes, comes in contact with the water. This precipitate was carbonate of lime, produced from the decomposition of the sulphate of lime in the water by the carbonate of potash.

Kermes is never of a good colour, when the solution is received from the filter in a cold vessel.

To determine the best relative proportions of sulphur and anti-

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mony, the following experiments were made. Sixteen grammes of antimony and 2 of sulphur, treated with 360 of potash and 4000 of water, yielded very fine kermes, rather deep-coloured, but light, brilliant, and brown, bordering a little upon the rose colour. Sixteen gr. of sulphuret of antimony and 8 of sulphur, yielded a white sulphuret of antimony instead of kermes. When the proportion of sulphur was still greater, no precipitation took place, unless an acid was added, and then the golden sulphur fell down.

On the other hand, 1 part of metallic antimony melted with 2 of sulphuret of antimony yielded very dark brown kermes, not velvety, but dull and tarnished. Equal parts of metallic antimony and sulphuret of antimony yielded a very deep, but tarnished kermes. The two last experiments being repeated with 9000 gr. of water, instead of 4000, the kermes was less beautiful.

To determine the proper time of boiling, a portion of the solution was poured off at the end of half an hour, and fresh water added to the remainder, so as to keep up the original quantity; this pouring off a portion and addition of water was repeated five times, at half an hour's distance each time. The four last produced kermes were nearly similar, but the first was much less tarnished, and dull; as it had some brilliancy, it was also much lighter.

As the sulphate of potash contained in the pearl-ashes debased the colour of the kermes, and was difficult to get rid of, on account of its being but very slightly soluble in cold water, some kermes was prepared with pearl-ashes, from whence the greater part of the sulphate had been separated by crystallization: this kermes was still dull and pale-coloured. Kermes made with calcined tartar resembled the common sort; that made with caustic potash was inferior to the ordinary kind: nor was the use of saturated carbonate of potash more successful.

The action of a solution of potash upon sulphuret of antimony being thus examined in every possible circumstance, it remained to try the action of soda. Crystallized carbonate of soda produced a most beautiful kermes, purple-brown, brilliant, and appearing like the shearings of velvet. This kermes was generally more beautiful when the water had boiled a few minutes before the carbonate was added.

Experiments similar to the preceding were tried with soda, but none gave such fine kermes as 16 grammes of finely-pulverized sulphuret of antimony treated with 360 of carbonate of soda, and 4000 of water, the boiling being continued for half, or three quarters of an hour, and the kermes washed with water that had been boiled.

It appears that kermes absorbs oxygen with the greatest avidity; and as neither sulphur nor antimony have this property, it evidently belongs to the hydrogen in the sulphuretted hydrogen contained in the kermes.

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When oxymuriate of lime was added to different portions of the alkaline solutions before the kermes was deposited, the kermes fell down paler, as the proportion of the oxymuriate was the greater, and became at last quite white. And when a solution of oxymuriate of lime was poured on kermes itself, it lost its colour by degrees, and at last became quite white. Oxymuriatic acid produced the same results.

Different proportions of a still turbid solution of hydroguretted sulphuret of potash, to which sulphuric acid had been just added to neutralize the alkali, was mixed with the alkaline solution of sulphuret of antimony; the kermes fell down darker coloured as the proportion of sulphur was the greater. A solution of caustic potash poured upon kermes instantly turned it yellow, and thus changed it into golden sulphur of antimony. On the other hand, the white kermes, said above to be obtained from super-sulphuretted antimony, was changed by liquid sulphuretted hydrogen to a yellow colour, and afterwards to a brown.

The colour of kermes is evidently caused by the sulphuretted hydrogen that it contains; but, to avoid all conjecture, it was resolved to have recourse to a regular analysis. As a mercurial pneumatical apparatus of sufficient size was not at hand, the quantity of precipitate obtainable from acetate of lead by sulphuretted hydrogen was employed as an indirect method of ascertaining the quantity of the latter; and it was found that a litre (quart) of sulphuretted hydrogen, weighing at 12°.5 Cels. and 757 millimetres of pressure 151 centigrammes, threw down 8 grammes of sulphuret of lead.

The analyses were then performed in an uniform manner. Muriatic acid was heated upon 10 grammes of the kermes, and the disengaged gas passed through a solution of acetate of lead; the sulphur was separated from the residuary solution of muriate of antimony by filtration, and washing with very dilute muriatic acid: the oxide of antimony was obtained by adding nitric acid to the dissolved muriate. In the following analyses the weight of this precipitate is stated; but it has been found that the precipitate is not a pure oxide, but a muriate with excess of antimony, as was shewn by treating it with carbonate of potash, and the solution retains some of the oxide by its excess of acid, as the addition of sulphuretted hydrogen gave it a brownish yellow colour. Hence there always appears a slight surcharge.

The finest kermes obtained by potash contained 20.31 per cent. of sulphuretted hydrogen, 7 of sulphur, and 82 of precipitated oxide, or rather submuriate of antimony.

Kermes from the mixture of sulphuret of antimony with its own weight of antimony, treated with potash, yielded 17.06 per cent. of sulphuretted hydrogen, 4 of sulphur, and 82.5 of oxide.

A fine specimen of kermes met with in a shop some years

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back, yielded 12.74 per cent. of sulphuretted hydrogen, 11 of sulphur, and 81.5 of oxide.

Fine kermes obtained by carbonate of soda yielded 21.62 per cent. of sulphuretted hydrogen, 2 of sulphur, and 83 of oxide.

Kermes obtained by carbonate of soda, but washed with unboiled water, yielded 20.56 per cent. of sulphuretted hydrogen, 25 of sulphur, and 85 of oxide.

Very pale kermes, which had been treated with oxymuriatic acid, yielded 0.49 of sulphuretted hydrogen, 16.5 of sulphur, and 83.5 of oxide.

White kermes from sulphuret of antimony and half its weight of sulphur, yielded 71 per cent. of sulphur, and 33 of oxide.

Hence sulphuretted hydrogen is evidently the colouring principle of kermes, and not a supposed marroon oxide of antimony, which does not exist; for on igniting white oxide ad medium, prepared by nitric acid, in a covered crucible, it only turned more or less yellow, but never of a marroon colour, although the operation was carried so far as to reduce some part of it to black oxide, or the minimum of oxygenizement. That part of the oxide thus treated which appeared to approach the nearest to a brown colour, was dissolved in muriatic acid; no hydrogen was disengaged, yet the solution yielded a white oxide when precipitated by water. Hence the colouration of the oxide by heat is only owing to its condensation, as the blueish white oxide thrown down by alkalies from solutions of copper turns of a deep brown on drying; and on the other hand, red oxide of quicksilver, when precipitated from acids, is yellowish.

It being desirable to separate the oxide from kermes without the alteration of its proportion of oxygen, which might have been occasioned by acids, a solution of caustic potash was heated upon kermes, a white oxide was obtained, which appeared by many experiments to be absolutely similar to that obtained from metallic antimony by nitric acid, and of course at a medium of oxygenizement. Nevertheless the oxide of kermes seems to be in an inferior degree of oxygenizement; for kermes is frequently produced, and in a large proportion, when emetic tartar is prepared from glass of antimony; and sulphuretted hydrogen throws down kermes from a solution of emetic tartar, as also from the butter of antimony. The oxide in the butter is the white volatile oxide; that in emetic tartar is less oxygenized; for when heated in a tube, part of it sublimes, and the remainder forms a gray oxide, that absorbs oxygen from the open air, and may then be sublimed. The oxide of kermes is evidently similar to that of emetic tartar, as it cannot be supposed that the attraction of sulphuretted hydrogen to the oxide is sufficient to cause the decomposition of water; and therefore the oxide obtained from kermes by means of caustic potash, was probably rendered more oxygenized by some of the water being decomposed, the potash taking

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up some of the sulphur of the kermes along with the sulphuretted hydrogen, and thus forming supersulphuretted hydrogen, which decomposes part of the water, the oxygen of the water being absorbed by the oxide of antimony.

Kermes dried in a retort without contact of air, became, when it was treated with potash, yellow, afterwards gray, and then very slowly changed to a grayish white oxide, which could not perhaps be brought to a perfect white, on account of the potash being highly coloured. At any rate, only a very minute portion of water is decomposed, and the oxide of kermes is very nearly the same as that of tartar emetic, and the volatile oxide of antimony, which according to Mr. Thenard differ from the medium of oxygenizement by a few hundredth parts of oxygen.

When fresh undried kermes is treated with caustic potash, it becomes yellow, and as soon as it is heated the kermes disappears, being entirely taken up by the hydroguretted sulphuret of potash that is formed.

The carbonate of soda here said to form the most beautiful kermes, is not the saturated carbonate, but that called by Berthollet, subcarbonate.

Observations.—The great use of kermes mineral in France, where it is equally in vogue with Dr. James's antimonial powder in England, renders this an interesting subject in that country, but of comparatively little use here. It is surprising that the tedious process of successive solutions made with small quantities of alkali at a time, should have been blindly followed for so long a time.

On the Distillation of Acetate of Copper, and on the Products obtained. By Messrs. Derosne.—*Ann. de Chim. No.* 189.

Chemists are still divided as to the identity or difference of radical vinegar with ordinary acetous acid; the opinion of Adet and Darracq, of their being identically the same acid, is however generally received, and appears to be the true theory.

In distilling verdigris it was observed that the last portions of radical vinegar that came over were lighter than the first, although they appeared by their penetrating smell to be the strongest.

Forty-one pounds and a half of distilled verdigris were distilled, and the acid separated as it came over into four portions. The total quantity of acid obtained was 20 lbs. 5 oz.; there remained in the retort 13 lbs. 14 oz. of oxide of copper, which on spontaneous combustion augmented in weight one tenth; of course the carbonic acid gas, gaseous oxide of carbone, and carbonated hydrogen gas, must have weighed 7 lbs. 5 oz.; but this included

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some acid, for the water through which they passed required 3 oz. of a concentrated solution of caustic potash to saturate it.

The first portion of acid weighed 5 lbs. 10 oz.; it had a weak acid smell, and which slightly coloured blue; it shewed $9\frac{1}{2}°$ by the hydrometer for acids, water being 0. It was rectified, and the product collected in three separate portions. The first shewed 7° hydr. and 30 grammes of it saturated 98 grammes of a solution of caustic soda, which shewed 14° hydr. The second shewed 8°, and saturated 101 gr. The third shewed $9\frac{1}{2}°$, and saturated 138.8 grammes.

The second portion of acid weighed 6 lbs. $4\frac{1}{2}$ oz.; its smell was stronger than that of the first, and it was deeper coloured: it shewed $10\frac{1}{2}°$ hydr. When rectified, the first portion shewed $9\frac{1}{2}°$ hydr. and saturated 180 grammes. The second shewed 10°, and saturated 196 gr. The third shewed $10\frac{1}{2}°$, and saturated 213 grammes.

The third portion of acid weighed 7 lbs. 14 oz.; its smell was stronger than the second, and empyreumatic; its blue colour was also deeper. It shewed + $4\frac{1}{2}°$ hydr. When rectified, the first portion shewed + 1° hydr. and saturated 180 grammes. The second shewed 5°, and saturated 215 gr. The third shewed 7°, and saturated 226 grammes.

The fourth or last portion of acid, which had been driven over by a violent heat, weighed only $8\frac{1}{2}$ oz.; it had a weak but very empyreumatic smell, was rather lemon-coloured, and did not contain any copper. It shewed + $\frac{1}{2}°$ hydr. When rectified, it was collected in only two portions. The first shewed + $2\frac{1}{2}°$ hydr. and saturated 64 grammes. The second shewed 6°, and saturated 112 grammes of the above soda.

From the apparent contradiction between the progressive density and power of saturation, the acid appeared to be combined with some substance lighter than water. The first portion, therefore, collected in the rectification of the third parcel of acid, and which was already 1° hydr. lighter than water, was redistilled; it yielded a gas of a sharp penetrating smell, which burned with a blue flame, and about 10 oz. of a liquor having the same smell, shewing + 10° hydr. for acids, equal to 20° hydr. for alkohol. The acid still existing in this liquid was separated by gradually adding pieces of caustic potash until it was saturated; the vessel being also plunged in cold water, frequently renewed, in order to hinder as much as possible the temperature from being increased, and thus losing the volatile product; crystallized acetate of potash fell down, and on distilling the supernatant liquor, which was of a citrine colour, it became perfectly white and transparent: its taste was hot, sharp, and empyreumatic; it was lighter than alkohol, shewing 48° hydr.; volatile; very combustible, burning at first with a blue flame, and afterwards with a yellowish white. It left after combustion some traces of charcoal. Burned with a

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little water, the residuum was slightly acid. It did not redden tincture of litmus. Miscible with water in all proportions; nevertheless it was evidently of an ethereal nature. Miscible also with nitromuriatic solution of gold; but on adding dri d muriate of lime, this ethereal liquid swam upon the saline solution, of a deep yellow colour, arising from the gold it contained.

This ethereal liquid might be called pyroäcetic, or oleoäcetic ether, to distinguish it from the true acetic ether, which is very different. It is evidently this kind of ether that diminishes the specific gravity of the latter portions of radical vinegar, renders it combustible, and distinguishes it from common acetic acid. It is also probable that this ether occasions the penetrating odour (montant), and favours the crystallization of the acid. Thus sulphuric acid is much more easily crystallized when saturated with sulphurous acid gas than before.

Some have thought that vinegar always retains some portion of alkohol; indeed when it is distilled in large quantities, an ethereal liquor comes over at first, from whence a true ether can be separated. But in the preparation of distilled verdigris, distilled vinegar is boiled upon oxide of copper, so that the alkohol must be volatilized. In decomposing the verdigris a weak uninflammable acid comes over first, accompanied only with carbonic acid gas; but when the salt has lost its water, the acid comes over concentrated with carbonic acid gas and carburetted hydrogen. This acid, which does not distil until the temperature is very high, is combustible, and must owe that property to some new product, and not to alkohol; that, even allowing its existence in the salt, must have been separated in the beginning of the operation.

Fourcroy observes, that alkohol is not indispensably required to produce ether; and ether may certainly be formed from its elements when they meet together, as is indeed the case in distilling verdigris. The formation of it is probably occasioned by the slight attraction of the acetic acid for copper, weakened as it is by the continual reduction of the copper by the charcoal deposited from the acid; whereas in the decomposition of the acetate of potash, the power of the alkali augments as the acid is disengaged.

These conjectures were confirmed by the examination of the fourth portion of radical vinegar; it was half a degree lighter than water: on rectification the first half was still lighter, and by saturation with caustic potash it yielded about half an ounce of ether similar to that above mentioned. The weakness of this portion is owing to the formation of water and a carbonaceous oil along with the ether; the oil being separable on redistilling the second half.

Observations.—It is evident from Mr. Cadet's paper on vinegar in the present number, that the use of the hydrometer is equally

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inapplicable to the ascertaining of the acidity of common vinegar, as there exists no analogy between the progress of the densities of the several vinegars he obtained, and their power of saturating potash.

On the Composition of the muriatic and acetic Ethers. By Mr. P. F. G. Boullay.—*Ann. de Chim. No.* 187.

Mr. Thenard, in his memoir, has made it a question whether the muriatic acid disengaged in such large quantities during the combustion of muriatic ether, is then formed, or merely set at liberty.

A thousand grammes (2 lbs.) of alkohol at 38° Baume's hydr. absorbed 680 gr. of muriatic acid gas, disengaged from dry muriate of soda by concentrated sulphuric acid. The alkohol was at the temperature of 10° Cels. (50° Fahr.), but it rose by the absorption of the gas to 24° (75° Fahr.). By this absorption the alkohol was rendered oily; its specific gravity was 1.134; without any colour; it emitted fumes into the air; was miscible with water, the temperature of the mixture being increased, and bubbles smelling of ether emitted. On distillation it yielded muriatic ether, which was washed either with water, or a solution of caustic potash.

Ten grammes (154 gr.) of muriatic ether were poured upon a solution of 10 gr. of caustic potash in as much water, and frequently blended together by agitation. In 48 hours the ether was perceptibly diminished, and being then left to evaporate spontaneously, the potash was mixed with nitric acid in excess: this acid salt precipitated nitrate of silver.

A similar solution of caustic potash was heated in a retort to 80° Cels. (176° Fahr.), and then 10 grammes of muriatic ether were introduced, the greatest part of which seemed to dissolve in the hot liquid. The apparatus being cooled, the liquid left in the retort was divided into two portions; from the first, vapours of muriatic acid were disengaged by sulphuric acid; and the second, supersaturated with nitric acid, precipitated nitrate of silver in a mass. There had passed over a colourless liquid smelling of potash, from which a small quantity of a liquid was separated by rectification, which had the smell and taste of rum. About a half litre (pint) of ethereal gas escaped the action of the potash.

Ten grammes of muriatic ether were almost totally absorbed by 25 of liquid ammonia, which on trial was found to have separated as much muriatic acid from the ether as the hot solution of caustic potash. When sulphuric acid was added, and the mixture distilled, some alkohol of a disagreeable odour passed over.

Muriatic ether was slowly decomposed by cold concentrated

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sulphuric acid; no colouration took place, nor was sulphurous acid disengaged. When the ether was passed through the acid while boiling, the mixture became black, and muriatic acid passed over; there was also collected an elastic fluid, composed of muriatic ether and olefiant gas, which burned with a red and green flame, and deposited much charcoal. These products preceded the formation of sulphurous acid.

Cold nitric acid at 36° hydr. did not seem to have any action upon muriatic ether, which continued to swim upon it. But when the ether was passed through boiling nitric acid, a small quantity of nitrous ether was formed, and much muriatic acid was separated; the nitric acid itself lost all its colour; and here it may be remarked by the way, that alkohol itself immediately discolours the reddest nitric acid.

These experiments were extended to acetic ether, produced by the direct action of acetic acid upon alkohol. This ether, on being passed through a hot solution of caustic potash, did not disengage any gas, but there passed over a liquor having a slight smell of acetic ether, burning like alkohol, and miscible in all proportions with water; then pure alkohol; and afterwards acetic acid, the disengagement of which was facilitated by adding sulphuric acid in excess to the residuum.

Acetic ether being added to sulphuric acid, the thermometer rose from 15° Cels. (59° Fahr.) to 60° (140 F.). On distillation, acetic ether with excess of acetic acid came over, and then pure sulphuric ether.

Sulphuric ether dissolved in acetic acid came over unaltered upon distillation.

From the above experiments it may be concluded that muriatic ether is a combination of muriatic acid and alkohol; the proportion of the acid being very large. Acetic ether appears also to be a combination of the same kind: hence ethers in general seem to be divisible into two classes; the first comprehending those in which the acid only causes the production, without forming an essential ingredient in the product: this class comprehends the sulphuric and phosphoric ethers. The second class comprehends the ethers produced by the volatile acids, and which seem to be combinations of the acid with alkohol in the manner of salts, the alkohol serving as a base; such as the muriatic, acetic, and probably nitric ether.

Observations.—The experiments of Mr. Boullay would seem to be decisive as to the muriatic and acetic acids being really contained in the ethers formed by those acids; but they are in one respect very defective, because they do not attempt to shew that the quantity of acid thus obtained is equivalent to that which appears, from the quantity of acid expended in the formation of the ether, to be contained in the portion decomposed.

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If in the preparation of sulphuric and phosphoric ethers, the acid only acts by abstracting a portion of water or its elements from the alkohol, the fluids are identical, and should be known by one name, with a temporary use of proper epithets when it is of any consequence to discriminate their mode of production.

On the Influence of galvanic Electricity on the Passage of Minerals from one Species to another. By Mr. Guyton.—*Ann. de Chim. No.* 188.

A native oxide of antimony found in Gallicia had so much the texture of native sulphuret of antimony, and even still retained in some parts the metallic brilliancy, that no doubt could exist but the molecules had undergone the slow action of some agent which had changed the composition of the fossil without altering its arrangement, as in the case of petrified wood.

This explanation, and the application of it to other fossils, as hepatic pyrites, gray copper ore, &c. have been developed in a memoir in the Journal de l'Ecole Polytechnique, Juillet 1802. And the ulterior views of Mr. Davy on the operation of electricity in the mineral system, led to the following experiments, in which Messrs. Hachette and Clement assisted.

A piece of sulphuret of antimony placed in a glass of water, was exposed to the action of a battery of sixty-four plates of copper and zinc, containing about 1260 square inches of surface, disposed in four piles, with pasteboard soaked in a solution of muriate of soda. The smell of sulphuretted hydrogen was perceived; the surface of the sulphuret turned of a deep yellow, and was rather iridescent. In four hours polished silver held over the liquor for a few minutes became black, and a drop of the liquor itself threw down a white precipitate from a solution of acetate of lead. The positive platina riband had a slight yellow crust; the negative one was black. In eight hours the battery was nearly exhausted, and the sulphuret was found to be covered with a yellow powder, which on being dried exhibited the same reddish-yellow colour as the native oxide from Gallicia, and it was difficult to discover a few remains of the metallic lustre of the sulphuret.

It cannot be supposed that a few hours should allow of a more perfect imitation of the natural process by which antimony in the bowels of the earth loses 17 per cent. of sulphur, and acquires 18 of oxygen, and which appears to be effected by water decomposed by galvanic electricity. It very rarely, or indeed never happens, that the same effect is produced by different causes, especially as the present effect bears every appearance of arising from one and the same cause.

A similar experiment was tried upon two other minerals, viz.

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hard, compact, very brilliant pyrites from Bérézoff, which is very poor in metal, and can be dissolved only in nitric and nitromuriatic acid, and gray silver ore (fahlerz) in crystals. In these experiments the same smell of sulphuretted hydrogen was perceived, the liquor became turbid, strongly acid, and precipitating acetate of lead, the platina ribands coloured as before, and the sulphurets were reduced nearly to a pulverulent state, and covered with a dull pellicle.

When the sulphuret of iron was operated upon, the conducting ribands were adjusted before the water was put into the vessel; and as soon as this was poured in, and covered the sulphuret, the latter immediately burst into a lively flame: a circumstance the more remarkable, as native transparent sulphur was not inflamed even in oxygen gas by galvanic electricity, although the battery was strong enough to burn iron wire.

These experiments point out a new method of desulphurating metals, which, although it cannot at present be employed in preparing ores for smelting, may admit of being applied to assaying.

On sparry Iron Ore. By Messrs. J. H. Hassenfratz *and* Descostils.—*Ann. de Chim. No.* 189.

These two gentlemen have engaged in a dispute on the subject of sparry iron ore. Mr. Descostils first published a paper, of which an abstract is given in our third volume, page 334. Ten months after this, Mr. Hassenfratz communicated to the Institute a memoir, in which he stated, that the fusibility of sparry iron ore could not be inferred from the size of its plates; that magnesia did not hinder the fusion, for a mixture of 100 parts of oxide of iron with 23 of magnesia was perfectly fusible, and several sparry ores could not be melted even with borax, although they did not contain magnesia. He also said, that at Allevard, when the smelting did not go on well, it was usual to add some large grained magnesiferous ore, and that the infusible ores were in fact only rendered fusible by their cohesion being diminished by the roasting and exposure to the air.

A month afterwards, Mr. Descostils gave in another memoir, of which an abstract was given in the last number. The committee that was appointed to examine these papers, on repeating the experiments, reported, that mixtures of magnesia and of oxide of iron, heated in a lined crucible, did not melt; the iron was reduced indeed, but the globules could not unite: the case is the same when the magnesian sparry ores are smelted; and that finely triturated iron ore from Elba did not melt easier than when only very coarsely powdered; while in both cases only a moderate fire was required.

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Upon this, Mr. Hassenfratz withdrew his memoir, and Mr. Descostils, supposing he had altered his mind, on publishing the above paper, cancelled the remarks on Mr. Hassenfratz's opinions: but he now again brings forward several of them in the form of a criticism upon Descostils' memoirs.

Mr. Hassenfratz says, that magnesia contributes in many cases to assist the fusion of the vein-stone, and to separate the metal. When one part of silica, lime, alumine, or magnesia, was mixed with four of suboxide of iron, moistened with oil, and heated in a lined crucible, the iron was reduced, but the grains remained separate. The case was the same when mixtures of the earths by two and two were heated with four times as much suboxide of iron. But on adding one part of magnesia to the mixtures containing silica and lime, or silica and alumine, the iron was collected into a single button. Mixtures of three earths in equal parts with six times as much suboxide of iron were tried. Those containing silica lime and magnesia, or silica alumine and magnesia, or silica lime and alumine, yielded a button; but the grains were not perfectly collected together in that containing lime, magnesia, and alumine: so that silica seems to act most powerfully as a flux to other earths. A mixture of all the four earths with the suboxide yielded a perfect button.

Sparry iron ores are always smelted with some of the vein-stone adhering to them, which contains silica, lime, and sometimes a little alumine; so that on analysing 12 specimens of scoria from Allevard, they were found to contain 46 to 71 per cent. of silica, 7 to 21 of lime, 1 to 8 of alumine, and 3 to 15 of magnesia.

It is this mixture of the ore with vein-stone that metallurgists ought to consider; but the vein-stones are frequently such as to produce an infusible compound, and in that case the smelters have found it convenient to add a mineral containing magnesia, as is practised at Allevard to promote the fusion; so that magnesia cannot be said to be the cause of the difficult fusion of sparry iron ore in the smelting furnaces.

As to the fusion of both kinds of sparry ores being promoted by adding magnesia, it has been since found that the refractory specimen contained 20 per cent. of silica and lime; and therefore it is probable that the assistant did not separate the vein-stone accurately enough, so that a fusible mixture of the three earths was formed. The other specimen with larger plates contained 8 per cent. of magnesia; and on adding a larger quantity, with borax, the magnesia was not completely melted, but adhered to the button, and even penetrated into it.

To ascertain the effect of the oxide of manganese in these ores, the following experiments were made:

Oxide of manganese, with four times as much suboxide of iron, was mixed with oil, and as much silica, lime, alumine, or mag-

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nesia as of the oxide, and melted in a lined crucible: that with silica yielded a button, in the other mixtures the reduced iron remained in separate globules. Similar mixtures containing silica and lime, or silica and alumine, or silica and magnesia, or lime and alumine, yielded buttons; but mixtures containing lime and magnesia, or alumine and magnesia, afforded only separate grains of iron. All the iron thus obtained was white, and the scoria olive-green, so that it retained the greatest part of the manganese.

When 1 gramme of silica, 1 of oxide of manganese, and 4 of suboxide of iron, were smelted, the button weighed 2.99 gr. and the scoria 1.70 gr. If no manganese had been used, the button would have weighed 2.9 gr.

Lastly, Mr. H. says, that metallurgists have always distinguished two kinds of roasting; the first, serving to get rid of sulphur, arsenic, or other volatile substances; and the second, to break down the ore, in order to facilitate its fusion, as the fire sooner penetrates small masses than large.

Mr. Descostils, on his part, observes, that as to ores holding magnesia being occasionally added to promote the fusion of difficultly fusible iron ore, the experiments of Mr. H. himself as to the fusibility of mixtures containing two or three different earths along with the oxides of iron and manganese, are sufficient to explain the circumstance.

The infusibility of certain sparry ores cannot depend entirely on the nature of the vein-stone, since the vein-stone not being uniformly distributed through the ores, these last would have some difference in their fusibility, which does not appear to be the case; neither could the vein-stones be separated by exposure to the weather. It is also doubtful whether these vein-stones always consist of silica and alumine; and again, those ores which do not contain much magnesia are in general rich in oxide of manganese, which, according to Mr. H.'s experiments, acts as a flux to mixtures of silica and lime, or silica and alumine.

It remains therefore to discuss, whether the magnesian ores are found unmixed in the veins, or accompanied with vein-stones, serving for a flux to magnesia. Mr. Souquet, director of a furnace at Allevard, says, that the ore at Allevard, when newly extracted, is of difficult fusion, and furnishes less iron than when old; and that the ore of Vaulnaveys and Vizille is of the kind called *maillat* ore, very magnesian. Mr. Treillard, formerly director of a furnace, says, the ore of Vaulnaveys and Vizille is richer than that of Allevard, and generally very refractory, but is sometimes accompanied with a kind of marble that renders it more fusible; it is also rendered easily fusible by exposure to the weather for four or six years, and mixing it with ore of Allevard. It is therefore evident that the ore of Vaulnaveys, although richer than that of Allevard, does not contain earthy substances enough to cause its

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fusion; and therefore it is necessary to add Allevard ore, which contains manganese and earthy substances, to serve as a flux.

The large foliated sparry ores *(maillat)* fall to powder in roasting, and frequently extinguish the fire: the imperfectly or confusedly foliated *(rives)* acquire great hardness, although they become black.

The opinions of Mr. H. as to the fusibility of ores being promoted by roasting, agree in effect with what had been stated by Mr. D. as the former explains himself to mean the shortening of the time, and not the diminution of the temperature.

Observations.—On considering the various papers, it is evident that Mr. Descostils has by far the best of the dispute. The objections of Mr. Hassenfratz are puerile, and seem only the mere dictates of envy directed against the growing merits of Mr. Descostils, who is rising in reputation as one of the first chemists and metallurgists in France. Although the nature of sparry iron ore is not yet absolutely cleared up, and the limits of the several species that have evidently been confounded together, are not exactly ascertained; yet the labours of Mr. D. have certainly thrown great light on them.

Chemical Experiments on soft Roe of Fish. By Messrs. Fourcroy *and* Vauquelin.—*Ann. de Chim. No.* 190.

Soft roe of carp is neither acid, nor alkaline; it does not yield ammonia by trituration with caustic potash, but forms a thick magma with it. Dried in the air, mixed with 1-5th of potash, and then moistened with water, it yielded only somes light traces of ammonia, arising from the muriate of ammonia contained in the roe. It loses 3-4ths of its weight by drying, becoming friable, and rather yellow. By heat it first hardens, then softens, and at last melts, exhaling yellow vapours smelling of animal fat. The charcoal of roe yields on elixiviation uncombined phosphoric acid, and the phosphates of lime, and of magnesia.

178 grammes of fresh roe produced 7.8 gr. of charcoal, which being calcined in a platina crucible, melted a hole in it, and rendered the metal brittle. The charcoal was reduced to 5 gr. by elixiviation. The ley deposited 0.45 gr. of phosphate of lime by evaporation, and the supernatant liquor deposited on saturation with volatile alkali 5 gr. of phosphate of ammonia containing a little phosphate of magnesia: 5.6 gr. of this ammoniacal phosphate distilled with a gramme of cork charcoal, yielded 0.26 gr. of phosphorus. The residuum still contained phosphoric acid; for on being washed with muriatic acid, the addition of lime-water to the acid threw down 4.5 gr. of phosphate, which contained a little phosphate of magnesia.

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123 grammes of fresh roe yielded on distillation a little carbonic acid gas, and carburetted hydrogen gas; much water containing carbonate, much prussiate, and some traces of muriate of ammonia; oil, at first white or slightly yellow, then blood-red but still fluid, and lastly, blackish-brown and thick; needle-like crystals composed of prussiate and carbonate of ammonia were condensed on the sides of the adopter, and a white crust, clouded with yellow and red, settled on the upper part of the adopter: this crust was pure phosphorus. The charcoal weighed 7.5 gr.; it was not acid, nor did it become acid by calcination; it also was not inflammable.

The oils thus obtained being treated with nitric acid at 30° hydr. disengaged white fumes smelling of phosphorus, and the mixture was luminous in the dark. On distillation there came over water holding carbonic, prussic, and muriatic acids, and a light-coloured oil not containing phosphorus: the residuum was partly a red bitter matter, partly a kind of wax which grew solid on cooling, and swam on the nitric solution. This residuary liquor being evaporated yielded crystals of nitrate of ammonia, and left phosphoric acid.

If the distillation of roe is not pushed on till the retort is white hot, phosphorus is not obtained, as it remains in the charcoal, which scratches glass, yields, by an obscure red heat, an intermittent greenish-yellow flame, and the residuum contains phosphoric acid.

Muriatic acid separates the phosphates of lime and magnesia from roe charcoal; and the residuum, on being again calcined, yields phosphoric acid, which of course does not proceed from the phosphates. Carbone strongly attracts phosphorus; for the same charcoal being calcined four times successively, yielded phosphoric acid. Roe-charcoal also contains the phosphates of soda and of potash, which are carried off by the water, as also azote; for when it is calcined with potash, it yields a prussiate.

Charcoal made from fibrine yielded an alkaline lixivium, so that the property of being acidified is not common to all animal charcoals.

Water ground along with fresh roe had the appearance of an emulsion, and remained turbid after filtration. On boiling albumen separated, and the residuary water precipitated infusion of galls, several metallic solutions, and formed a jelly on evaporation, which being burned and calcined, did not yield any phosphoric acid; but sulphuric acid extracted from the charcoal, the phosphates of lime and of magnesia. Water boiled on fresh roe did not take up from it any phosphate of ammonia, which confirms the existence of uncombined phosphorus in roe. The albuminous matter separated from roe as above stated, being burned, left a charcoal similar to that of the whole roe, and therefore contained the phosphorus.

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Alkohol extracted from roe a soapy matter of a disagreeable smell and taste, and left the roe dry and rough to the touch. The alkohol being distilled became greenish-yellow, and left a residuum resembling soap in smell and taste.

Margraff alone has said that phosphorus was contained, ready formed, in organic bodies, and mentioned mustard-seed as an example; which however does not appear to yield the smallest trace of it.

Perhaps it is this uncombined phosphorus that occasions the phosphorescence of fishes; and the same cause may produce the phosphoric appearances of many marine animals and terrestrial insects.

Observations.—The existence of uncombined phosphorus in the soft roe, or male sperm of fish, is a very curious fact, especially when connected with the extraordinary aphrodisiac powers of phosphorus itself, given internally in very minute doses.

The accuracy of Margraff has hitherto been unquestioned, and therefore we cannot allow any French chemist, not even Vauquelin himself, to come forward at this time and impeach his veracity. It is evident from the text, that a very violent heat is requisite to extract phosphorus from roe: now the common distilling furnaces of the French are so thin, that the greatest part of the heat escapes through their sides; and their chimnies are so short, that even when made of bricks they do not produce the temperature of the furnaces used in the north of Europe, or in England. Hence nothing is more common than for the chemists of France and Italy to esteem as nonentities those products of the German and other northern chemists, which require a very great heat for their production. It is very likely that the distillation of phosphorus from mustard-seed may require a greater heat than if roe is employed: indeed it may be observed in general, that although an accurate chemist may be allowed to affirm what substances can be distilled from any body by the temperature he has employed, he is no judge of what may be produced by a higher temperature.

On the Combination of Phosphorus with Metals and their Oxides, in the moist Way; with the Examination of Gas that arises from a particular Manner of decomposing Alkohol, and on the Light of the Italian Glow-worm. By Mr. Theodore de Grotthuss.—*Ann. de Chim. No.* 190.

When alkohol which has been digested upon phosphorus is heated in a dark place, there arises from the phial a flame 5 or 6 inches in height, which does not burn the hands, or any other substance, however long they may be held in it.

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Phosphuretted alkohol poured into solutions of gold, silver, quicksilver, or copper, immediately throws down a phosphuret of those metals, and a smell of ether may be perceived. The phosphurets of silver, or quicksilver, are deep-coloured; that of gold is darker than the purple powder of Cassius, and more resembling indigo: this precipitate applied on porcelain, and heated, gilds it very finely. The other phosphurets dissolve in nitric acid, disengaging nitrous acid, and leaving the phosphorus behind; they also are phosphorescent on hot iron, and leave a spot of metal on it.

It is very singular, that if a few drops of phosphuretted alkohol are let fall into a solution of gold in a flat dish, the particles of metal which are revived move with great rapidity in different directions; and this spectacle is rendered more beautiful by the iridescence of the metallic particles. Cold stops this motion, but warmth brings it on again.

The precipitates obtained by adding phosphuretted alkohol to solutions of tin, lead, bismuth, antimony, or platina, only differ from those thrown down by the alkalies, or sometimes by alkohol, by containing some phosphorus mixed with them; but phosphorus may be chemically combined with the oxides of these metals, and then they vary extremely.

Six parts of alkohol were boiled upon two of phosphorus and one of caustic potash; on cooling, a fine red oxide of phosphorus was deposited, and the liquid was left perfectly clear. This alkohol charged with phosphuret of potash throws down a very bright orange precipitate from a solution of nitrate of lead; a movement takes place in the liquid, and black brilliant crystalline grains of lead holding a little phosphorus appear. Alkohol charged with phosphorus, or with caustic potash, only throws down a white precipitate from the same solution. The oxide thrown down by the alkali did not become orange when treated with phosphuretted alkohol.

The orange precipitate sometimes turns of a darker colour, and sometimes becomes much whiter; the latter change may be avoided by washing it in cold water; it then preserves its brownish-red tint. It exhales a garlic smell by rubbing, and gives out the same scent, accompanied with a beautiful phosphorescent appearance, by being thrown on a hot iron, leaving upon the iron phosphate of lead mixed with white oxide: it does not effervesce with acids, but yields the nauseous scent of phosphuretted hydrogen. Nitric acid dissolves part of it, no nitrous gas is disengaged, and phosphorus is left. Hence this substance is a hydroguretted phosphuret of oxide of lead. After being exposed for some time to the air, the oxide imbibes carbonic acid, and then nitric acid disengages a gas from it, which might be taken for the nitrous, but which differs from it in smell, and in wanting the reddish cast.

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Alkohol boiled upon phosphorus and pure soda precipitates nitrate of lead of a deep colour; if this precipitate is rapidly dried by blotting-paper, it detonates on being struck by a hammer, and the paper in which it is folded up contains revived lead: it also detonates on being touched with a single drop of sulphuric acid.

Water, alkalies, or alkohol which have been digested upon phosphorus, precipitate nitromuriate of antimony of a white colour; but alkohol charged with phosphuret of potash throws down a fine brown powder, resembling kermes mineral. This being washed and dried without heat remains unchanged, is phosphorescent by heat; it is partly soluble in muriatic acid, emitting a garlic scent, and leaving phosphorus; it does not decompose nitric acid, and is therefore a hydroguretted phosphuret of oxide of antimony.

Although alkohol charged with phosphuret of potash precipitates all metallic solutions, these precipitates are sometimes mere oxides mixed with phosphorus, as in solutions of tin, zinc, cobalt, and manganese; sometimes phosphurets of the metal, and of course capable of decomposing nitric acid, as in solutions of silver, quicksilver, copper, and bismuth. Besides the above two phosphuretted oxides, it is probable that others may be discovered. The proportion of phosphorus and alkali in the alkohol has great effect even upon them; for if either of those ingredients predominate, the precipitate, whether it contains lead or antimony, is white.

Common sulphuret of potash precipitates nitrate of lead red; but if the alkali is in excess, the precipitate is white, and it is black if the sulphuret is sufficiently hydroguretted, and the metallic solution has not an excess of acid to disengage the sulphuretted hydrogen.

Nitrate of quicksilver often yields a precipitate with alkohol, charged with phosphuretted alkali, which when washed with cold water fulminates by percussion; but it is more certain to mix phosphuret of quicksilver prepared from phosphuretted alkohol with dry crystals of nitrate of quicksilver.

Phosphuretted alkohol throws down black, brilliant, and crystalline grains of phosphuret of lead from an alkaline solution of oxide of lead.

Paper which has been written upon with solution of gold, silver, quicksilver, or copper, being introduced into a bottle containing a piece of phosphorus, or in which phosphorus has been left for a few minutes, the letters appear in a short time with a metallic brilliancy, and may be made more beautiful by being burnished.

Phosphuretted alkohol is an excellent mean to deprive water containing oxygen of that gas, as, on exposing the mixture to solar light, the phosphorus becomes oxidized, but not acidified, and the water may be made limpid by filtration.

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When alkohol is digested as above mentioned upon phosphorus and a caustic fixed alkali, it is decomposed, water is formed, and a phospho-carburetted hydrogen gas is emitted.

This gas is invisible, has a garlic smell, but differs from phosphuretted hydrogen by not inflaming on mere contact with air, or oxygen, not even at the temperature of 70° Reaum. (189° Fahr.): when set on fire, it deposits phosphorus; and detonates, if mixed with oxygen. It is not altered by nitrous gas; but if oxygen is added to the mixed gases, a flash of light appears, and a most violent detonation takes place. Three measures of oxymuriatic acid gas being gradually introduced to one of this gas, it burns at each addition with a green flame, without any explosion: the mixture is first very much dilated, and afterwards as greatly diminished. This gas retarded the germination of the seeds of cresses.

Fifty measures of this gas were mixed with 100 of oxygen, and set on fire by the electric spark over lime-water: after the combustion a diminution of 80 measures had taken place, no phosphorus was deposited; and on the residuum being washed, 6 measures more were absorbed; the remaining 64 were almost pure oxygen, for they were entirely absorbed by combustion with hydrogen. The precipitate from the lime-water was soluble in nitric acid with effervescence; but caustic ammonia poured into this solution threw down phosphate of lime. Hence 50 measures of this gas require 36 of oxygen for their combustion, and this produces water, phosphoric acid, and carbonic acid. Trommsdorff obtained a gas composed of hydrogen, phosphorus, and carbone, by heating phosphoric acid upon charcoal, which was perhaps of the same nature as the above described gas.

The alkohol that has been boiled upon phosphorus, and a caustic fixed alkali, does not contain any phosphate, as appears on adding it to lime-water; but if the boiling has been continued for a long time, so that phosphuretted hydrogen has been disengaged, which is known by the spontaneous inflammation of the bubbles as they rise, the liquid contains more water than alkohol, and a little phosphate; so that the alkohol has been decomposed, all its carbone, and most of its hydrogen being abstracted from it, and only water left. The residuary phosphorus preserves its liquid form at 10 (54° Fahr.), and even lower; but when flung into water, or on any cold body, it congeals, and takes the appearance of the finest amber, particularly when soda was employed.

On the Phosphorescence of the Lampyris Italica.

The lampyris Italica is a glow-worm that is very abundant in summer all throughout Italy. Its phosphorescence is not diminished for the first quarter of an hour that it is detained under water, and it requires several hours immersion to extinguish it. In oil its phosphorescence ceases in fifteen or twenty minutes: it is also

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equally extinguished by hydrogen, carbonic acid gas, and nitrous gas. If withdrawn a short time after the light is extinguished, the light reappears; oxygen will also revive it when atmospheric air will not; but if the insect, either alive or dead, is exposed to the reddish vapour of nitrous acid, or to a mixture of oxygen or atmospheric air with nitrous gas, the phosphorescence immediately reappears of a greenish colour, and lasts for about a minute, after which it cannot be made to reappear.

Observations.—This paper makes us acquainted with some curious combinations of phosphorus, and not only adds to our knowledge of that singular substance, but also to that of metallic precipitates.

On Hydrosulphuret of Soda; and on Improvements that might be made in the Manufacture of Soda. By Mr. FIGUIER.—*Ann. de Chimie, No.* 190.

Vauquelin discovered by accident hydrosulphuret of soda in the mother water of soda as prepared by Mess. Payen and Bourlier, which arose in his opinion from their not using a sufficient quantity of carbonic oxide to saturate all the sulphur proceeding from the decomposition of sulphate of soda by charcoal.

Soda obtained from the combustion of vegetables also contains hydrosulphuret of soda; for on saturating a solution of Alicant soda with tartarous oxidule, the odour of sulphuretted hydrogen was very perceptible.

A concentrated solution of common soda was evaporated, and a considerable proportion of carbonate of soda separated by crystallization; the mother water was left alone for a month, and then it was found to contain crystals of hydrosulphuret of soda mixed with carbonate of soda, as appeared by numerous trials. These crystals were partly rectangular tetraedral prisms terminated by four-sided pyramids, and partly octaedrons with rhomboidal bases.

Muriatic acid poured upon common soda disengaged sulphuretted hydrogen gas.

Pulverized crystals of tartarous acid mixed with those of soda also disengaged sulphuretted hydrogen gas.

These experiments were also made with the soda sold under the names of Carthagena varec and salicor, and with the same result.

When the plants are burned, the sulphates contained in them are probably decomposed by the charcoal: in consequence of the heat being too great, the disengaged sulphur combines with the alkali, and also with the hydrogen yielded by the plant or the

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water it contains, and thus a hydrosulphuret of the alkali is formed.

The sulphuret and hydrosulphuret of soda remain in the mother water when carbonate of soda is prepared from barilla, or kelp, and of course a great loss of alkali is sustained. They also diminish the causticity of the ley in soap-making, especially as they are not decomposed by lime. In the art of dyeing their existence in soda must be detrimental to the colours, and to the bleaching. And they may also occasion accidents, for it is customary for soap-boilers to keep the ley in covered cisterns: here the sulphurets decompose water, and the disengaged hydrogen mixing with the air in the void part of the cistern, has sometimes exploded on the cistern being opened, and a light brought near the opening, to enable the workmen to look into the cistern.

Another inconvenience arising from the excessive heat employed in the manufacture of soda is the vitrification of a large proportion of it with the earthy particles in the plants, by which it is rendered insoluble in water or acids.

This error of calcining the ashes of the plants too strongly, probably arose from the barilla, or kelp, being mostly used for the manufactory of glass, in which the extraneous substances abovementioned that form 4-5ths of the kelp are of no injury, except that they augment the expense of carriage.

It would, therefore, be more profitable to follow the same method in preparing kelp as in preparing potash, to elixiviate the ashes, to evaporate the ley to dryness, and sell it in this state.

Observations.—That the process recommended by the author would be an improvement in the manufacture of barilla, or kelp, is certain, but it would, perhaps, be very hard to find a vent for his improved article, which is commonly more difficult to be accomplished than the mere improvement of any substance which has been long used in the arts.

Theoretical chemists are not sufficiently aware of this circumstance, and should consider that there are in general only two practical modes of improving a chemical manufacture:—the first, by bringing into the market an article of equal goodness, under the same name, and at a reduced price: the second, by offering a superior article for the same price. Very few articles will succeed if the price be raised, even if superior in goodness. An attention to these circumstances would tend to prevent many disappointments.

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On tartarous Acid, and particularly on the Acid yielded by Tartar, when it is distilled. By Messrs. FOURCROY *and* VAUQUELIN.—*Journ. de Phys. Nov.* 1807; *and Ann. de Chim. No.* 190.

FOURCROY and Vauquelin some time ago stated that the acids denominated pyroligneous, pyromucous, and pyrotartarous, were only acetic acid combined with empyreumatic oil. Mr. Gehlen, however, attacked this statement, because the acid liquor obtained from cream of tartar by distillation, yielded crystals by a gentle evaporation, which could not be acetic acid, and were certainly not ordinary tartarous acid.

Fourcroy and Vauquelin therefore repeated their experiments, and those of Gehlen and Rose, and found that when the acid liquor obtained from tartar by distillation was saturated with carbonate of potash, a brown resin was precipitated, but a large portion of the oil remained united with the acid, which being evaporated to dryness, and purified by several successive solutions in water, yielded a brownish salt, of a hot sharp taste, and the scaly appearance of acetate of potash. This salt precipitated the nitrates of quicksilver and silver in white scales, but it also precipitated acetate of lead. By heat it swells up, and becomes carbonized. When distilled by a gentle heat with diluted sulphuric acid, it first yields a very acid liquor, which has only a slight smell of vinegar; the residuum then becomes black, and yields at last a white sublimate.

The acid liquor that came over, contained a large globule of a heavier yellow fluid, which had the appearance of melted phosphorus; it was put by for examination, but, during the night, the globule was taken up by the supernatant liquor.

The sublimed crystals were very acid; they melted quickly on any thing hot; yielded a white fume, and left no residuum. They dissolve for the most part in water, and again crystallize by spontaneous evaporation. The solution does not precipitate nitrate of silver, but it precipitates nitrate of quicksilver. Acetate of lead is not immediately precipitated, but after some time acicular crystals disposed in feather-like forms, are deposited.

This sublimed acid being dissolved, and a little potash mixed with it, does not yield an acidulous salt like tartar, but it then immediately precipitates acetate of lead. When the potash is sufficient to saturate the acid, the combination is deliquescent, soluble in the alkali, and does not precipitate the salts of barytes, nor of lime.

The distilled liquor that comes over with this sublimed acid,

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yields on evaporation crystals possessing the properties of the sublimate.

This pyrotartarous acid is, therefore, a peculiar acid, and differs as well from the acetic as from the tartarous. The acetic acid is more volatile, has a more powerful smell, and, when combined with potash, it does not precipitate acetate of lead. The tartarous acid precipitates the acetates of lead, lime, and barytes; it forms a difficultly soluble salt with potash. Neither does the pyrotartarous acid resemble any of the other vegetable acids.

This acid does not owe its origin to an union of acetic acid with the oil of tartar formed at the same time; for on distilling concentrated acetic acid with this oil, afterwards combining it with potash, and then decomposing this salt by sulphuric acid, nothing but an empyreumatic vinegar was obtained.

It was, however, shewn by Grosse, that a combination of tartarous acid and lime, being kept with water in a well-stopped bottle, was changed into acetate of lime in a few months.

It is probable that a very small quantity of acetic acid is formed in the distillation of tartar, on account of the sharp and penetrating smell that is exhaled when sulphuric acid is poured upon the combination of the distilled acid with potash.

The acids formerly called the pyromucous and pyrolignous, still appear on further examination to be merely alterations of acetic acid by empyreumatic oil.

The formic acid is not a combination of the acetic and malic acids, as was formerly supposed, but it consists of the acetic acid mixed with a combination of phosphoric acid and an animal matter, which gives that acid some of the properties of the malic.

The purest acidulous tartarite of potash (cream of tartar) yields by distillation, besides the acid liquor and charcoal, 35 per cent. of dry carbonate of potash, 0.6 of tartarite of lime, 0.12 of silica, 0.025 of alumine, 0.075 of iron mixed with manganese, besides slight traces of the sulphate and muriate of potash. Crude tartar, or argol, contains still more of these extraneous substances.

Observations.—Some credit is due to Fourcroy, for having retracted his former opinion respecting the pyrotartarous acid; and we apprehend he will find cause to do the same with respect to the pyromucous acid, or that yielded by sugar when distilled, which has always appeared to us to differ considerably from vinegar.

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On the Opium that is commonly sold, and on that extracted from Papaver somniferum, grown in France, free from its poisonous and stupifying Odour. By Mr. ACCARIE.—*Ann. de Chim. No.* 192.

THE opium met with in the shops, contains an oily matter resembling wax, which is supposed to arise from the pollen of the poppy, and to be the cause of its deleterious narcotic properties. It also contains a substance that is analogous to albumen, or, according to other authors, to gluten, and to which Mr. Josse attributes the stupifying odour; a crystallizable substance, a small quantity of caoutchouc, and the remains of different vegetables.

Whatever may be the origin of the deleterious property of opium, it may be got rid of by charcoal. Twenty-four ounces of choice opium are to be macerated in rain-water for 12 days in the dark, stirring it twice or thrice a day The water being poured off, fresh water is to be poured on the residuum, and macerated for four or five days. The liquor, being mixed and filtered through flannel, is to be boiled for a full quarter of an hour with 2lb. of charcoal powder, and again filtered; clarified with the whites of two eggs; refiltered, and slowly evaporated to a proper consistence for forming pills: and thus 2 oz. of extract will be obtained free from the disagreeable smell of the opium.

The liquor preserves its transparency during the whole of the evaporation, and does not deposit the least sediment.

The part of the opium that did not dissolve in water, is equal in weight to the extract obtained; it gave a deep brown colour to alkohol, and water threw down 6 drachms of resin in reddish flakes. The residuum was then boiled with distilled acetic acid, and the solution being evaporated, it yielded 6 drachms of resino-extractive matter with very little smell. The residuum boiled in water, yielded 2 drachms of extractive matter, which did not burn by itself.

These three products weighing 14 drachms, were mixed together, and treated again with alkohol, to separate the resin; but as it still coloured water, the resin was boiled with that fluid. It then weighed only 2 drachms, was friable, but became flexible by being worked in the hands; on burning, it yielded a thick smoke, had no taste, but still retained the peculiar smell of opium.

Two pounds of dried poppy-heads (without their seeds, which alone weighed 2½lb.), being broke to pieces, and boiled in rain-water, yielded, after filtration and evaporation, 10½ oz. of an extract resembling common opium. This extract, treated with alkohol for three months, yielded to that solvent, 1 oz. 6 drachms,

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which was not resin, for the alkoholic solution was not rendered turbid by water, but was certainly the most efficacious part of the extract. The residuary 9 oz. left by the alkohol, being again dissolved in water, yielded 6 oz. of an extract less bitter than before, with a saline taste, and which might be used instead of opium by using four times the usual dose.

In 1800, being a dry season, there was collected from the stalks of 200 plants in a walled garden of a southern aspect, from whence the heads had been taken before they were perfectly ripe, about half an ounce of a blackish-red opium in tears, very bitter, and not so strongly scented as common opium. This opium resembled the *gobaar* of the Orientals.

On Bones found in an old Tomb at Paris. By Messrs. Fourcroy *and* Vauquelin.—*Ann. de Chim. No.* 191.

These bones are supposed to have been interred in the 11th century, or 700 years ago. They are very brittle, particularly the heads of the bones. Their colour is purple, like dried wine-lees, but the heads are brownish; they contain a quantity of white brilliant crystals resembling sulphate of lime, which have separated the laminæ of the bones, and caused their brittleness.

The bones being pulverized, and boiled in 300 times as much water, rendered it acid, and of a fine red colour. Ammonia destroyed the redness, and threw down a green precipitate, which became blueish when dried, and was principally composed of ammoniaco-magnesian phosphate. The bones themselves lost 35 per cent. and much of their purple colour. They then dissolved in nitric acid without any effervescence, leaving a small quantity of white powder mixed with a few brown membranes. The part taken up was phosphate of lime with some red colouring matter.

A piece of bone put into diluted nitric acid turned it red, and left a red soft substance retaining the form of the bone, which being treated with alkohol, turned it also of a fine red, as would have been done by alkanet, and left only some brown flakes, which had escaped the action of the agents employed; these flakes being burned left only a few grains of sand.

The colouring matter of these bones is therefore soluble in alkohol, and even in water; alkalies give it a green colour, resembling that of some kinds of rotten wood. But although rotten wood renders alkohol dark purple, it becomes green again on adding acids; while the colouring matter of these bones, after being changed green by alkalies, becomes red again on adding acids. This red matter is very similar to one formed by the putrid decomposition of the gluten of flour.

The white brilliant crystals found in these bones appeared upon

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examination to be acid phosphate of lime holding a small quantity of phosphate of magnesia. In fresh bones the phosphoric acid is by no means in excess, and is even accompanied with carbonate of lime; hence, during the decomposition of the animal matter of the bones, either some acid has been formed which has not only decomposed the carbonate of lime, but even taken away some lime from the phosphate of lime, and left the remainder with excess of acid; which is improbable, as neither the acid nor the lime is to be recognised in the mass; and it is unlikely that they were both of them volatilized together; or some phosphorus existed in the animal matter which has been converted into phosphoric acid, and then combined with the phosphate of lime to form an acid phosphate.

In this case the quantity of this phosphorus must have been considerable, as the phosphoric acid has also taken away the lime from the carbonic acid. The acid phosphate of lime has been already found in the intestinal calculi of herbivorous animals, with a very distinct crystalline form; but the excess of acid was not so great, nor the salt consequently so soluble in water.

The preservation of the red colouring matter during so many ages is to be attributed, perhaps, to its union with phosphate of lime.

Observations.—This paper is to be considered as a continuation of the discovery of the existence of phosphorus in animal substances, first stated in Fourcroy and Vauquelin's paper on the soft roe of carp, of which an abstract is given in our present number.

On some peculiar Properties of Charcoal. By Professor L. Brugnatelli.—Ann. de Chim. No. 192.

It has been already stated (see Retrospect, vol. ii. p. 254), that hydrogen was chemically combinable with charcoal by means of galvanism. Several chemists had already shewed that charcoal absorbed different gases when it was plunged red-hot into them; but the properties of the charcoal were not altered, and the gases could be expelled by a heat below that of boiling water.

Hydroguretted charcoal may be prepared still easier than with a galvanic apparatus, by plunging red-hot charcoal into water; part of its hydrogen combines with the charcoal, and the other part is disengaged in the form of carburetted hydrogen.

Charcoal may also be oxidized by the galvanic fluid from the positive pole being passed through charcoal under water; or still more readily by plunging charcoal into oxymuriatic gas. The temperature is not raised, and no carbonic acid is formed; but the

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oxidized charcoal must be washed with water in order to separate the adherent muriatic acid.

Concentrated nitric acid may also be employed to oxidize charcoal; in which case very pure nitrous gas is disengaged, on applying heat to the acid and charcoal. Charcoal thus oxidized may either be left exposed to the air till it has no action upon litmus, or the remaining nitric acid may be got rid of by a solution of potash. Light has a considerable effect in the oxidizement of charcoal by nitric acid: charcoal moistened with nitric acid being exposed to the sun, gave out nitrous gas, and was converted into oxidized charcoal. Pieces of charcoal from whence the interposed air had been driven by their being soaked in water acidulated with nitric acid, were put under a bell filled with the same water, and exposed to the sun: nitrous oxide was disengaged for three days, the water lost its acidity, and the charcoal was oxidized.

Charcoal is augmented in weight by being oxidized; it has no taste, nor are its physical properties sensibly altered; its electric power being examined by means of a condenser and an electrometer, with a plate of zinc, indicated 8°; common charcoal indicated 4°; and hydroguretted charcoal half a degree.

Common charcoal and hydroguretted tried upon the tongue, the latter tasted acid, and was positive; when common and oxidized charcoal were tried, the common was positive. Prepared frogs were convulsed when common charcoal put under their back was brought into contact with the thighs, covered either with hydroguretted or oxidized charcoal.

Volta has established the following scale of solid electrometers: oxidized charcoal, oxide of gold, crystallized black oxide of manganese, plumbago, common charcoal, gold, silver, platina, copper, brass, pinchbeck, bronze, metallic antimony, tin, lead, tinfoil, hydroguretted charcoal, zinc.

Common charcoal was tied by a silk thread to a plate of zinc, and hydroguretted charcoal to a plate of lead. The plates were kept in separate vessels of water for eight hours, temp. 16 De Luc, or 66 Fahr. The plates were oxidized, and no gas was disengaged until the charcoal was disoxidized; then part of the hydrogen united with the charcoal, and the remainder was disengaged.

Galvanic piles formed of common and hydroguretted charcoal separated by moistened charcoal, were of very weak power, and their action of very short duration. Greater success was obtained by using common and oxidized charcoal, separated by disks moistened with water.

The facility with which charcoal in contact with water may be made to separate either the hydrogen or oxygen from it, according to the pole of the galvanic apparatus where it is placed, seemed

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to shew that one of Ritter's charging piles might be constructed with common charcoal.

A pile of 120 disks of common charcoal, separated by moistened charcoal, was put upon the positive pole of a metallic pile, and the upper part of the charcoal pile was made to communicate with the negative pole of the metallic: in a half hour's time the charcoal pile was separated from the other, and was stronger than that described by Ritter. Charcoal surpasses in this respect even gold itself: but the effect of the secondary pile is of very short duration, as the hydroguretted charcoal soon loses its hydrogen, and becomes of no use.

Further experiments were also made to compare charcoal with metals.

Charcoal negatively galvanized in an ammoniacal solution of gold, became gilt; it was also covered with a coating of pure copper by being galvanized in an ammoniacal solution of copper, or in a solution of nitrate of copper; and with fine silver in an ammoniacal solution of silver, or a solution of nitrate of silver. Similar effects were produced in several other metallic solutions.

The action of the pile is not necessary for these precipitations; for if oxidized charcoal in contact with a plate of zinc is held for a few minutes in metallic solutions, the charcoal becomes covered with the metals.

The Count of Morozzo advanced that charcoal decomposed water at the ordinary temperatures of the atmosphere by the help of light; but the gas thus disengaged is only the air contained in the charcoal, and in the water itself.

On the Action of vegetable Acids on Alkohol, both without and with the Intermedium of the mineral Acids. By Mr. THENARD.—*Memoires d'Arcueil, Tom.* ii. 1809.

SCHEELE was the first chemist who studied this subject, and till now no one has resumed it: he discovered that acetic ether and oil of benzoin might be formed by distilling the acetic and benzoic acids respectively with alkohol mixed with one of the mineral acids; but his attempts to unite these two acids directly with alkohol were as unsuccessful as his endeavours to form combinations of the other vegetable acids with alkohol, by means of an intermedium.

Mr. Thenard having considered Scheele's researches, judges them, from their inferences, to be incomplete, and therefore thought it necessary to repeat them.

Following the example of Scheele, he divides his memoir into two parts: the first treats of the action of pure vegetable acids on alkohol; the second of the action of the same acids assisted by a mineral acid.

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Of all the vegetable acids soluble in alkohol he found acetic to be the only one altered by repeated distillations with it; this by a few operations completely disappeared, and afforded a true ether.

But he ascertained when a portion of mineral acid is added to some of the vegetable acids, and distilled with alkohol, that new and singular products are formed.

Exp. 1. Four hundred and sixty-three grains (30 grammes) of benzoic acid dissolved in 926 gr. (60 grms.) of alkohol were distilled with 231 grs. (15 grms.) of concentrated muriatic acid in a glass retort, connected with an appropriate apparatus; the distillation was stopped when nearly two thirds over; the last portions distilled contained a substance that, purified from benzoic and muriatic acid by warm water, was of a yellowish colour, rather heavier than water, pungent, fusible at a temperature from 25° to 30°, volatile at about 80°, acid, oily, almost insoluble in cold water, rather less insoluble in boiling water, soluble in alkohol, from which water precipitates it; when freed from an excess of acid by potash, it is white, odorous, and liquid; it is decomposed by long agitation with potash into alkohol and benzoic acid; and though it contains no sensible quantity of muriatic acid, yet without the aid of this acid it cannot be procured.

Exp. 2. By distilling a mixture of 463 gr. (30 grms.) of oxalic acid, 656 gr. (36 grms.) of alkohol, and 154 gr. (10 grms.) of strong sulphuric acid, till sulphuric ether began to be formed, a brownish liquor of great acidity was obtained, that deposited on cooling only crystals of oxalic acid; water separated from it a substance similar to the oil of benzoin.

Analogous products were obtained by treating the citric and malic acids in a manner similar to the preceding. They differ from each other principally in their taste: the one from oxalic acid has a slight astringent taste; the one from citric acid an acerb taste; the taste of the other is unknown. The first alone is volatile; rather more so than water. They are separated by distillation with caustic potash into alkohol and their respective acids. Sulphuric acid acts in the formation of these compounds the same part as the muriatic does in that of the oil of benzoin. A compound of a peculiar nature was formed by distilling gallic acid with alkohol and sulphuric acid.

Alkohol and sulphuric acid mixed with tartaric acid in the same proportions as before with the oxalic, and distilled till ether appeared, yielded a thick syrupy substance, from which nothing peculiar could be separated, as in the preceding instances, by water. It has a brownish colour, and is slightly bitter; it is very nauseous, inodorous, free from acidity; largely soluble both in water and alkohol; it does not precipitate muriate of lime, but abundantly muriate of barytes; during calcination it emits a strong smell of garlic, and leaves only a carbonaceous residue, containing much sulphate of potash. It is found similar to the preceding bodies in

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composition. Its syrupy state, and the property it has of rendering sulphate of potash soluble in alkohol, are its most peculiar characters.

In the following experiments alkohol of the specific gravity of .800 was used, and acetic acid that crystallized at 0.

1st Exp. Some grains of acetic ether were formed by distilling with a strong heat a mixture of 463 gr. (30 grms.) of alkohol and 308 gr. (20 grms.) of acetic acid.

2nd Exp. The last experiment repeated, with the addition of 67 gr. (5 grms.) of concentrated sulphuric acid, afforded with singular facility, and almost without any heat, 617 gr. (40 grms.) of acetic ether. This is an excellent process for the manufactory of ether, and far superior to the one in use. The purification is easily effected by decanting the liquor after being digested with a little potash.

An excellent ether may also be economically made by distilling to dryness 3 parts of acetate of potash, 3 of concentrated alkohol, and 2 of concentrated sulphuric acid, and by redistilling the dry mass with one fifth its weight of the most concentrated sulphuric acid. The quantity of ether produced is equal to the alkohol employed. The other acetates may be used, but the proportions of alkohol and sulphuric acid must then be varied.

More both of the nitric and muriatic acids are required than of the sulphuric, to aid the conversion of equal quantities of alkohol into acetic ether. Phosphorus, arsenic, and oxalic acids assist the formation of this ether; but the same effect is not produced by sulphurous acid gas, or by the tartaric and phosphoric acids.

From these facts Mr. Thenard concludes that the efficacy of the auxiliary acids is proportionable to the power each has of condensing alkohol. Hence strong sulphuric acid, which possesses this property in an eminent degree, is used with greater success than any other; but when diluted with water, it at the same time loses its power of condensing alkohol and of aiding the formation of acetic ether.

The following principle expresses in a general manner the results of the preceding experiments.

None of the pure vegetable acids except the acetic can form direct chemical combinations with alkohol; but assisted by mineral acids capable of condensing alkohol, they unite with it, and the mineral acid is excluded from the compound.

Mr. Thenard conceives that this principle may include animal acids, and probably extends itself to the mineral ones; he thinks that by its application means may be found of uniting not only these acids with alkohol, but even all animal and vegetable substances with concentrated acids. The inquiry is, according to him, a field fertile in results, and abounding in new sources for the combination of matter.

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Observations.—The new facts relative to the combinations of the oxalic, citric, and malic acids with alkohol, constitute the principal merits of this paper. Some things we find brought forward as the discoveries of the author which were well known before. Of this description is the method of forming acetic ether without the assistance of a mineral acid; our readers will find in Aikin's Chemical Dictionary, under the article Ether, that the honour of the first discovery of this method belongs to the Count de Lauraguais, and that Pelletier procured by frequent distillations of equal parts of acetic acid and alkohol half as much ether as there was alkohol employed. It is not correct that Scheele is the only chemist who has applied himself to this subject; it will be seen by referring to the same book, that, besides the names of the two inquirers just mentioned, that of Bergmann deserves to be noticed.

Mr. Thenard has presented this order of compounds in a new point of view; and reasoning on well-grounded analogy, considers them as only a part of a series of similar bodies, that alkohol may form with all the acids when placed in peculiar circumstances.

The new facts in this paper, when farther investigated, will probably lead to the true theory of the formation of ethers, which, notwithstanding the elucidations advanced by many celebrated chemists, still remains very obscure.

On the Combination of Acids with animal and vegetable Substances.
By Mr. THENARD.—*Memoires d'Arcueil, Tom.* ii. 1809.

MR. Thenard's former inquiries naturally led to the question, whether the property of combining with, and neutralizing acids, does not belong to all vegetable and animal substances. Analogy answers it in the affirmative. To resolve it, the following experiments were made:

Alkohol and oxymuriatic gas were reciprocally decomposed by passing the latter through the former. The products were large quantities of water, of an oily matter, and of muriatic acid, with minute portions of carbonic acid, and of a carbonaceous matter. The oily matter alone was subjected to experiment.

Purified by water and potash it does not redden paper of *tournesol*; its colour is white; its taste cooling, like that of mint; its smell is peculiar, but not ethereal; its weight and volatility are greater than those of water; it is very soluble in alkohol, but not in water; it is decomposed by distillation with nitric acid, but the products vary according to the strength of the acid: if weak, much muriatic acid is produced, and little oxymuriatic: if strong, the contrary. Its decomposition is effected too by passing it through a red-hot iron tube. But its decomposition by alkalies is very difficult. Hence it appears that the basis of this compound has a

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great affinity for acids, on which it acts the same neutralizing part as the alkalies. Mr. Thenard conceives that it contains much carbone.

Among vegetable substances some of the essential oils possess in the most eminent degree the property of combining with acids. Oil of turpentine absorbs nearly one third its weight of muriatic acid gas, and is converted into a crystalline substance; Kend discovered it; Tromsdorff and some French inquirers studied its nature; Gehlen is the last who has examined it. All these chemists, except Gehlen, judging from its appearances, consider it an artificial camphor, differing from the oil in composition by a loss of hydrogen and oxygen in the proportions to make water, and also by a loss of a little carbon.

Gehlen, on the contrary, reasoning from the facts, supposes that the essential oil is decomposed, that the principal part of its hydrogen combines with a small portion of carbon, and with a certain quantity of muriatic acid to constitute the camphoric concretion, whilst the other principles of the oil unite themselves with the muriatic acid remaining, to form a blackish-brown liquid.

Mr. Thenard found that 1544 gr. (100 grms.) of the purified essence of turpentine, placed in a cooling mixture, absorbed 463.2 gr. (30 grms.) of muriatic acid gas; the product was a soft crystalline mass, that by filtration for three days separated into two parts; a liquid containing numerous crystals, weighing 308.8 gr. (20 grms.) and 1698.4 gr. (110 grms.) of a white substance, crystalline, granular, and volatile, smelling sensibly of camphor; by exposure to air it lost the power of reddening turnesol paper, and warm alkaline leys with difficulty abstracted any more acid. It afforded, when decomposed by strong nitric acid, much oxymuriatic acid; and when by diluted, much muriatic acid.

The liquid from the crystalline mass was white, and emitted acid fumes, but by exposure to the air it ceased to produce this effect, and lost its acidity; it crystallized at a temperature below zero, and contained much muriatic acid in intimate union.

Mr. Thenard objects to the theory of Gehlen on the formation of this compound: he conoludes that the essential oil is not decomposed by muriatic acid, but that the two bodies are combined together in certain proportions that form the concrete camphor: his reason is, the evident analogy between oil of turpentine and alkohol, the principles of which unite themselves to the same acid without offending more different products.

Mr. T. conjectures that the camphor extracted from the essential oil of plants is formed by the union of the essential oil with an acid, and that the camphor of commerce is a compound of an essential oil and vegetable acid.

Mr. T. formed combinations of the essence of lemon and la-

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vender with muriatic acid. The first was brown and concrete; the latter acid, blackish, and liquid.

The property of combining with acids does not solely belong to the essential oils; many of the fat oils possess it, and afford with acids species of soap.

Tannin is a vegetable substance that combines with acids; it is precipitated by sulphuric acid from its solution in water, and the precipitate formed is a compound of sulphuric acid and tannin. In the nut-gall the gallic acid is retained by a true affinity that water cannot overcome; for, however often the nut-gall is washed, it will retain the power of reddening turnesol paper. As the quantity of gallic acid afforded by spontaneous decomposition far exceeds the free acid, it is natural to conclude that the excess is neutralized by the tannin. The increased acidity, in the solution, as the decomposition proceeds, might lead to the belief, were there not many reasons to the contrary, that tannin is transformed into gallic acid. First: it is known that acetic acid is the only one afforded by vegetables during spontaneous decompositions. Second: nut-galls evidently contain gallic acid; as these bodies have an affinity for each other, they should become neutral in part. Analogy favours this idea. Third: tannin precipitated from a solution of nut-galls contains gallic acid; for it produces nearly the same effects on the metallic solutions as gallat of potash. Thus it gives red precipitate with solutions of per-oxides of mercury, &c. It follows, then, that we are ignorant of the pure tannin of nut-galls, and of the part taken by it when acting on metallic solutions.

There are five animal substances known to possess the property of combining with acids, viz. curd, albumen, picromel, gelatin, and urea. Coagulated milk is curd united to an acid, but the affinity of the two bodies is so weak, that they may be separated by a large quantity of water. The union of albumen with acids is much more perfect, but the nature of the compound varies according to the strength of the acid. The concentrated acids produce on it the same effect as boiling water, and render it insoluble in diluted ammonia. Acids much diluted precipitate it, but it redissolves when deprived of the acid, both in water and weak ammonia.

Picromel readily combines with acids, forming precipitates that redissolve when the acids are neutralized.

Urea, according to MM. Fourcroy and Vauquelin, is precipitated from water by nitric acid alone; but probably other acids may combine with it, and form soluble products. These compounds can never be made except the temperature is above 40 or 50°: the operation is attended with the extrication of carbonic acid gas, and the production of much ammonia.

Gelatin unites principally with oxymuriatic acid gas. By passing this gas through a solution of gelatin a slight action is per-

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ceived, and pearly filaments of great elasticity and flexibility gradually subside. Mr. Bouillon Lagrange was mistaken in supposing these filaments to be oxygenated gelatin; they are really compounds of gelatin slightly altered with the muriatic and oxymuriatic acids. Their striking properties are insolubility both in water and alkohol; imputrescence, slight acidity, spontaneous emission of oxymuriatic acid for several days, solubility in alkalies, with which they form muriats.

From the preceding facts it is very probable that all animal and vegetable substances may either directly or indirectly be united to acids. Researches which have these compositions in view are of the greatest importance, as introducing us to the knowledge of a long series of compounds of a particular order, and as likely to throw great light on animal and vegetable analyses. It is likely that many organic bodies are of this kind. Is not the nut-gall an example? May not those bodies which afford acetic acid by distillation contain it ready formed? May not amber be a compound of an oily matter and succinic acid? Lastly, may not both the bitter principle and yellow acid procured by treating animal substances with nitric acid, be compounds of a peculiar matter and the acid? In a note attached to the end of the volume, Mr. Thenard relates, that Mr. Chevreul has proved in a paper read before the Institute, April 17, 1809, that this is really the case with the first of these compounds, and that the nitric acid and peculiar matter are united so strongly as to be separated alone by destructive distillation.

Notice, too, is here taken of a new ether, the pyro-acetic, procured by Mr. Chenevix, by the distillation of metallic acetats with muriatic acid.

Messrs. Thenard and Roord formed a precipitate composed of acid and animal matter by adding liquid oxymuriatic acid to water in which wool had been boiled.

Messrs. Thenard and Chevreul are of opinion, that the artificial tannin of Hatchett, and the charcoal of Proust treated with nitric acid, are most probably only compounds of vegetable matter and acid.

Observations.—This paper contains many interesting details, but few original observations. All the combinations of tannin here noticed, with acids, have long been in the possession of English chemists: Mr. Davy made them known with many others, including those with oxides, alkalies, and earths, in a paper on tannin in the Philosophical Transactions, as early as 1803, where he anticipates Mr. Thenard's conclusions, that nut-galls contain chemical compounds of gallic acid and tannin, and that the tannin of nut-galls is not pure.

The idea of Messrs. Thenard and Chevreul, that the artificial tannin of Mr. Hatchett is merely a compound of vegetable mat-

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ter and nitric acid, is, we are of opinion, hypothetical and groundless. The phenomena which are observed to take place during the conversion of charcoal into tannin, are in themselves evidences perfectly demonstrative of its fallacy. According to Mr. Hatchett the conversion is attended with a brisk effervescence and emission of nitrous gas. This certainly indicates the decomposition of the acid, and not its combination with charcoal.

Mr. Thenard objects to the theory of Gehlen on the formation of the artificial camphor: the only reason he advances is, the analogy between the formations of ethers and this substance; but to have rendered this even plausible, he should have first proved that alkohol itself is not decomposod during its conversion into ether; till this is done we must confess that Gehlen's explanation seems to accord with the phenomena much better than Mr. Thenard's.

The Bakerian Lecture.—An Account of some new analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, carbonaceous Matter, and the Acids hitherto undecompounded; with some general Observations on chemical Theory. By Humphry Davy, *Esq. Sec. R. S. F. R. S. Ed. and M. R. I. A.—Phil. Trans. Part I.* 1809.

The objects which have principally engaged Mr. Davy's attention are, the elements of ammonia; the nature of phosphorus, sulphur, charcoal, the diamond; and the boracic, fluoric, and muriatic acids.

On the Action of Potassium on Ammonia; and on the Nature of these Bodies.

It was stated in the Phil. Trans. for 1807, that by treating potassium in ammonia, hydrogen and nitrogen were produced, and the potassium appeared to be oxidated.

In a later communication to the Phil. Trans. experiments on the amalgam from ammonia are detailed, and an opinion of Lussac and Thenard relative to the agency of potassium and ammonia is controverted, even on their own statements.

Retorts of green glass were first used in operating on ammonia by potassium; but as they contained metallic oxides, those of plate-glass were substituted, and afterwards the potassium was first inclosed in trays of platina or iron. The retorts were exhausted by an excellent air-pump, filled with hydrogen; exhausted a second time, and then filled with very pure ammonia. All the operations were performed out of the contact of every body that might interfere with the results. The potassium principally used was obtained by the method of Lussac and Thenard,

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which differs but little from that produced by electricity, and is much less expensive.

Potassium introduced into a retort containing ammonia is soon covered with a crust of potash, there is a slight diminution of gas, and a little hydrogen is produced. By the heat of a spirit-lamp the potassium passes through shades of blue and green, and is at last entirely converted into a dark olive substance. In this process ammonia disappears and hydrogen is produced, as is stated by Lussac and Thenard. The quantity of ammonia absorbed seems to depend on the moisture it contains. Thus when the gas was saturated with water, 8½ gr. of potassium occasioned a loss of only 12½ cubical inches, but there were 16 cub. in. when the gas had been exposed for two days to ignited potash.

The quantity of inflammable gas generated seems to be uniform in all cases when equal quantities of metal are used, and to be rather less than is produced by the action of potassium on water.

The properties of the substance produced by the action of potassium on ammonia are thus described.

It is crystallized in irregular facets, and in colour and lustre is not unlike the prot-oxide of iron; it is opaque when in large masses, but semi-transparent when in thin films, and appears of a bright brown colour.

It fuses just above 212°, and if the heat is much increased it evolves gas. It sinks rapidly in oil of sassafras. It is a non-conductor of electricity. When melted in oxygen gas it burns vividly, oxygen is absorbed, nitrogen emitted, and potash which seems to contain water is formed. Water acts on it with energy, and often produces inflammation, ammonia is absorbed, hydrogen evolved, and the substance disappears with a hissing noise.

Almost all the ponderable matter of the ammonia that disappears exists in the olive substance. In an experiment when the ammonia was very dry, 6 gr. of potassium had gained more than 2 gr. by the process:—greater precision was prevented by the rapidity with which it absorbs moisture.

In an experiment when 8 grains of potassium had absorbed 16 cub. in. of ammonia in a retort, the olive substance gave off 12½ cub. in. of gas *, when heated nearly to redness, and of this ¼ of a cub. in. was ammonia, and the residuum, when mixed with oxygen, in the proportion of 6½ to 6, and fired by the electric spark, diminished to 5½.

Numerous experiments have been made by heating the olive substance inclosed in metallic trays, in polished iron and platina tubes, and the results have always been analogous, with such

* The average of six experiments made in a tube of iron is 2.4 of inflammable gas to 1. of uninflammable. The average of three made in green glass retorts is 2.3 to 1.

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slight differences, as might arise from temperature and other accidental circumstances.

In all cases the results have been very different from those of Lussac and Thenard, who, in consequence of the presence of moisture, obtained more ammonia and less of the mixed gases. Could an exact quantity of moisture be added, there is reason to believe that the ammonia generated would exactly equal the quantity which had first disappeared.

The olive substance, after being heated to a dull red, was examined under naphtha, as it is instantly destroyed by exposure to air. Its colour is black, and its lustre scarcely inferior to plumbago. It is opaque, very brittle, gives a deep gray powder, and is a conductor of electricity.

When raised to a low red heat, it does not fuse, but blackens plate-glass, and a gray sublimate rises, which produces the same effect on glass.

By exposure to air it usually takes fire, and burns with a deep red light. With water it heats, effervesces violently, evolves ammonia and hydrogen, and potash remains.

It has no action on mercury. By heat it combines with sulphur and phosphorus; the compounds are very inflammable, and by water emit ammonia, sulphuretted hydrogen, and phosphuretted hydrogen.

This peculiar substance, on the antiphlogistic theory, ought to be composed of potassium, a little oxygen and nitrogen, or of a suboxide of potassium and nitrogen; for the hydrogen disengaged in the operations nearly equalled the whole quantity contained in the ammonia employed.

Many experiments were made to ascertain the quantity of ammonia generated by the action of moisture on the olive substance procured in the usual way from quantities of 6 grains each of potassium. In one case $4\frac{1}{2}$ cub. in. were obtained; in others, when a small excess of water was suspected, not so much ammonia was produced.

The quantity of oxygen absorbed, and of nitrogen produced, were ascertained by filling vessels containing the olive substance with oxygen gas, over mercury. It often inflamed spontaneously, and always burnt at a low heat. In one of the most accurate of the experiments, $2\frac{1}{2}$ cub. in. of oxygen were absorbed, and only $1\frac{1}{10}$ cub. in. of nitrogen evolved. But a similar portion of olive substance would have furnished a quantity of ammonia by water, which, if decomposed by electricity, would have produced at least $2\frac{1}{4}$ cub. in. of nitrogen.

When oxygen was gradually supplied to the olive substance, by heating it in vacuo with the oxides of lead and mercury, in no case did the nitrogen exceed $1\frac{1}{4}$ cub. in.

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Here then is an evident loss of nitrogen: had it entered into any new combinations?

With a view to enlighten the inquiry, an experiment was made with great precaution on a portion of the olive substance in a tube of platina: in this case the utmost heat of a portable forge was applied. There were, making the proper corrections, $3\frac{1}{2}$ cub. in. of gas obtained. Twelve measures of it, and 6 of oxygen, were detonated, and the residual gas was $8\frac{1}{2}$, and contained oxygen. This experiment was repeated on different quantities with similar results.

In the lower part of the platina tube there was found potash apparently pure, and on the upper part, some potassium: water occasioned a violent inflammation, but gave no smell of ammonia.

From the extraordinary nature of this experiment, some source of error was imagined. Nitrogen only was expected, but a gas was obtained which diminished much more by detonation with oxygen than that from ammonia decomposed by electricity.

Similar experiments conducted with equal care were made with iron tubes, and the results were perfectly analogous. In one case $2\frac{1}{2}$ cub. in. of hydrogen were generated by the action of water on the revived potassium indicating at least $2\frac{1}{2}$ grs. of metal.

By Mr. Davy's calculations, if the analysis of ammonia by electricity approaches to accuracy, in the processes detailed, there is a considerable loss of nitrogen, and a production of oxygen and inflammable gas. And there is an apparent generation of nitrogen in the experiment of the action of water on the residuum.

Allowing the data to be correct, the decomposition and composition of nitrogen seem evident. Oxygen appears to be one of its elements, and a gas, possessing the properties of hydrogen, the other. But is it a new species of inflammable gas? or has nitrogen a metallic basis which alloys with iron or platina? or is water the ponderable matter of nitrogen, hydrogen, and oxygen? or is nitrogen a compound of hydrogen with a larger proportion of oxygen than exists in water? Of these important questions the two first, from the results, seem least probable.

Analytical Experiments on Sulphur.

The experiments of Clayfield and of Berthollet, jun. seemed to shew that sulphur in its common form contained hydrogen: its non-conducting power was opposed to the developement of its elements by Voltaic electricity; yet the intense heat produced by the battery might occasion some changes in it.

Some experiments were made on this idea in 1807. A curved glass tube, with a platina wire hermetically sealed in it, was filled with sulphur, melted by a spirit lamp, and acted on by a battery of 100, 6-inch plates: brilliant sparks were produced, and a little

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sulphuretted hydrogen was evolved. But this experiment was not conclusive, as Dr. Thomson has shewn that sulphur in certain forms may contain water. It was now repeated with Sicilian sulphur sublimed in a retort filled with nitrogen, and kept hot until it was used. A battery of 500, highly charged, was employed. The action was most intense, and the light very brilliant, and after some time, a quantity of sulphuretted hydrogen was generated; and the sulphur, from a pure yellow, became of a deep red brown tint, and reddened litmus paper.

Other experiments seemed to shew that there was no limit to the production of this gas.

The experiments on the union of sulphur and potassium prove the energy with which they combine. Sulphuretted hydrogen is produced with intense heat and light.

There is a strong analogy between resinous and oily substances and sulphur. But those bodies consist of a little hydrogen and oxygen with a large proportion of carbonaceous basis. The existence of hydrogen in sulphur is fully proved.

The change produced by electrized sulphur on litmus paper might be referred to the sulphuretted hydrogen formed; but even this gas is an evidence that sulphur contains oxygen.

In former experiments, when small globules of potassium were heated in large quantities of sulphuretted hydrogen, sulphuret of potash was produced; but as water might have been present, no conclusion was formed.

When these experiments were repeated with larger quantities of potassium, the effects indicated the presence of oxygen; white fumes appeared, the potassium inflamed, and burnt with great brilliancy; the residual gas was principally hydrogen, with a little sulphur in solution. The solid matter, by the action of diluted muriatic acid, produced sulphuretted hydrogen, but in less quantity than would have been generated, had the potassium merely combined with combustible matter.

Now if sulphuretted hydrogen contains oxygen, its existence must also be allowed in sulphur; and the intense action of sulphur on potassium and sodium may be referred, not only to the affinity of these metals for its basis, but also for its oxygen.

These opinions appeared to be confirmed by the results obtained by heating potassium and sulphur in hydrogen. In cases when the quantity of sulphur was smallest, to a given weight of potassium, the greatest quantity of gas was obtained, and in no instance did the quantity of gas equal the hydrogen, which the potassium alone would have generated by its action on water.

From these facts sulphur may be considered as composed of a little oxygen and hydrogen, and a large portion of basis which produces the acids of sulphur in combustion.

It is probable that in metallic combinations the sulphur still re-

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tains its oxygen, and part of its hydrogen, as they are only partially decomposed by heat, and the sulphur obtained appears to be merely common sulphur.

Analytical Experiments on Phosphorus.

The experiments on this body are similar to those on sulphur, as they are analogous in their properties.

Common electrical sparks passed through phosphorus produced no permanent gas; but when acted on in a similar manner to sulphur with a battery of 500, phosphuretted hydrogen-gas was evolved, and the phosphorus became of a deep red-brown colour.

In cases when potassium was heated in phosphuretted hydrogen gas, no inflammation occurred; white fumes appeared, and some gas was absorbed; what remained did not spontaneously inflame, but held a little phosphorus in solution. The phosphuret acted on by diluted acid, gave phosphuretted hydrogen.

These experiments seemed to indicate the presence of oxygen in this gas; but in the action of potassium on phosphorus, the evidence is more direct.

In fusing one grain of each of these bodies together, they combined with intense ignition, phosphuretted hydrogen was evolved in the process, and afterwards, by the action of diluted acid. When one grain of potassium and 3 of phosphorus were used, much less gas was obtained than in the former case, nor can the deficiency be well referred to any cause, but the supply of oxygen to the potassium from the phosphorus.

On the States of the carbonaceous Principle in Plumbago, Charcoal, and the Diamond.

The accurate researches of Allen and Pepys prove, that these bodies by combustion give similar quantities of carbonic acid, and absorb similar quantities of oxygen; hence they must principally consist of the same elementary matter; but the differences in their properties may be owing not only to the arrangement of their parts, but also to differences in their chemical nature.

In experiments on plumbago it was very intensely ignited by a battery of 500 in a Torricellian vacuum, but it remained unaltered, and no gas was evolved. When heated with potassium the results were similar to those obtained by exposing potassium to similar circumstances, and it gave no evidence of the presence of oxygen.

When charcoal from the willow, intensely ignited, was electrized in a Torricellian vacuum, with every precaution to avoid moisture, the results were very different from those of plumbago; the effect was far more brilliant, and gas was evolved which detonated with oxygen, and produced carbonic acid. The char-

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coal became harder at the point, and its lustre approached to that of plumbago.

In experiments when potassium was heated with charcoal, there were no indications of oxygen.

The diamond, from its nature, could not be acted on by Voltaic electricity.

When a diamond is heated in a green glass tube with potassium, there is no intense action, nor any gas evolved; but it blackens, and gray scales appear to be detached. In experiments of this kind, the results gave less hydrogen than potassium would have afforded in similar circumstances.

In one case, when the process was continued for three hours, the diamond lost $\frac{2}{100}$ of a grain; and the black matter obtained, burnt when connected in the Voltaic circuit; and carbonic acid was formed by its combustion in oxygen.

These results seem to shew that plumbago, charcoal, and the diamond, consist principally of the carbonaceous element; but in plumbago it is combined with about $\frac{1}{20}$ of iron; in charcoal, with a little hydrogen, and in diamond, with a minute quantity of oxygen.

Experiments on the Decomposition and Composition of boracic Acid.

In a former experiment (Phil. Trans. 1808), this acid seemed to be decomposed by Voltaic electricity, and a dark-coloured inflammable substance appeared on the negative surface. Many attempts have since been made to collect this substance for examination.

When boracic acid, moistened with water, was acted on by a power of 500, an olive-brown matter slowly formed at the negative pole, and at last appeared almost black. It was permanent in water, but soluble with effervescence in warm nitrous acid. When heated to redness on platina, it burnt slowly, and emitted white fumes, which slightly tinged litmus paper, and left a black mass, which, when examined by a glass, seemed vitreous, and evidently contained a fixed acid. These facts appeared to prove the decomposition and recomposition of boracic acid. But as the peculiar substance was a non-conductor of electricity, and could only be obtained in minute films, it was not possible to examine its properties with accuracy.

Other methods of decomposition were adopted. Boracic acid was heated with potassium in a gold tube, borate of potash was formed, and a dark-coloured matter, similar to that produced by electricity. About two months after this experiment had been made, and while Mr. Davy was yet examining the results, he was informed that Mr. Thenard had decomposed boracic acid by potassium in a copper tube.

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It was found, by various experiments, that 20 grains of potassium had their inflammability quite destroyed by about 8 grains of boracic acid.

For collecting quantities of the olive-coloured substance, tubes of iron, copper, and brass, were used after being exhausted. In all cases the acid was decomposed, and the products were analogous. The tubes were usually heated to redness.

The new substance is separated from the compound by washing. It appears as a pulverulent mass of a dark olive. It is opaque, very friable, and does not scratch glass, nor conduct electricity. When dried only at 100° or 120°, it gives off moisture by an increase of heat; and below the boiling point of olive-oil, it burns with a red light, and scintillates like charcoal. It does not appear to be fused, volatilized, or decomposed, by an intense heat, but its specific gravity is a little increased.

It burns brilliantly and scintillates in oxygen gas by a gentle heat, and boracic acid is formed. The peculiar substance exhibits some beautiful phenomena in oxymuriatic gas. It instantly burns with a brilliant white light, and a white film of boracic acid is formed, and a black matter remains, which in a fresh portion of gas, inflames by a gentle heat, and produces boracic acid. This inflammable substance was scarcely altered when heated nearly to redness in hydrogen and nitrogen. It rendered strong nitrous acid red, nitrous gas was produced and absorbed, while the substance dissolved rapidly by heat, and the fluid contained boracic acid. It only acted on strong sulphuric acid by heat, a slight effervescence was occasioned, and a deep brown solution was formed, which, by potash, gave a black precipitate. It gave muriatic acid by heat, a faint tint of green, but little of the matter was dissolved. It had no action on acetic acid. Its combinations with the fixed alkalies gave dark precipitates by muriatic acid. When kept long in fused sulphur, it slowly dissolved, and the sulphur acquired an olive tint. Phosphorus had scarcely any action on it, nor did it combine with mercury by heat. These facts shew that the combustible substance obtained from boracic acid by potassium, differs from all known species of matter, and seems to be the same as that procured from it by electricity, and the decomposition and recomposition of the acid seem to be fully established. From Mr. Davy's calculations, which he regards only as approximations to accuracy, boracic acid consists of 1 of the inflammable matter, to about 1.8 of oxygen.

Mr. Davy conceives that the olive substance is not an elementary body, as its properties are similar to those of primary compounds that contain oxygen.

Some experiments were made on the olive substance with potassium and iron, with a view to deoxygenate it, but the results were not decisive. The substance which alloys with potassium

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and iron, is considered as the true basis. In the olive substance, the basis seems to exist in union with a little oxygen, and when dried at common temperatures, contains water. In the black non-conducting matter, produced by the combustion of the olive substance, the basis is evidently combined with more oxygen, and when fully oxygenated, is boracic acid.

From the general properties of these oxides, and from all analogy, Mr. Davy regards the boracic basis as metallic in its nature, and has named it *boracium*.

Analytical Inquiries on fluoric Acid.

It was stated in a former paper (Phil. Trans. 1808), that potassium burns by heat in fluoric gas, and much gas is absorbed.

Numerous experiments have since been made to ascertain the results with precision. In one of the most decisive, 10½ grains of potassium were heated in about 19 cubic inches of gas; there was a brilliant inflammation, and, after the process, 14 inches had disappeared, and about 2¼ of hydrogen were evolved. The bottom of the retort was lined with a dark-coloured substance, which, when heated in air, burnt slowly, and became a white saline mass. When heated in oxygen, it burnt with difficulty, and oxygen was absorbed. It acted violently on water; inflammable gas was evolved, and some chocolate particles separated, which burnt below a red heat in oxygen, and became white; oxygen was absorbed, and acid matter produced.

From the minute quantities of this substance hitherto obtained, Mr. Davy has not gained decided evidence that the inflammable part of it, is the pure basis of the fluoric acid, though it can scarcely be doubted that the acid is decomposed by potassium, and regenerated by its combustion in oxygen.

As the chocolate substance always afforded silex by combustion, and even after being boiled in concentrated fluoric acid, it probably is a compound of the siliceous and fluoric bases in a low state of oxygenation.

Other experiments, as far as the results have been examined, appear to corroborate the above details.

Experiments on muriatic Acid.

The experiments made on this body have been more numerous than on any of the others which have been mentioned. When muriatic acid gas, made as dry as possible, is electrized, or heated in potassium, the results afford no indications of decomposition, but lead to the conclusion, that, in its common state, it contains at least $\frac{1}{3}$ of its weight of water.

A number of experiments were made to obtain the acid free from water. Mixtures of dry muriate of lime and sulphate of iron; and dry muriate of lime, phosphoric glass, and boracic

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acid, were heated to whiteness in a luted porcelain retort: the results proved that the gas could not be obtained without moisture.

The fuming muriate of tin, which contains dry muriatic acid, was distilled with sulphur, and with phosphorus, but without success. Triple compounds were obtained, which did not redden litmus paper, and copiously evolved muriatic gas by moisture.

In other cases, mixtures of corrosive sublimate and sulphur; and of corrosive sublimate and phosphorus, were distilled, and when dried by a gentle heat, scarcely any muriatic gas was evolved, but small quantities of fluids were obtained (analogous to those of Dr. Thomson, and Lussac and Thenard), which did not redden dry litmus paper; but by moisture, the gas was copiously evolved. These compounds are conceived to contain dry muriatic acid.

Numerous other experiments were made, but no uncombined acid could be procured. The effects of potassium were tried on the singular compounds formed in the attempts to procure the dry acid.

In cases when potassium was brought in contact with a little of the fluid, obtained by the action of phosphorus on corrosive sublimate, by gentle heat, it burnt brilliantly, and the retorts were often destroyed. There was no apparent evolution of gas; a solid mass remained of a greenish colour and dark gray. It was extremely inflammable, and often burnt spontaneously by exposure to air; in water it violently exploded, and gave a smell like that of phosphuretted hydrogen, and the residuum contained muriate and phosphate of potash.

In attempts to perform this experiment in an iron tube, the apparatus was soon destroyed, and when potassium was heated in the vapour of the compound, the inflammation was most intense, and the retorts were broken or fused. The solid residuum appeared to be the same as that described.

In cases when the sulphuretted compounds were acted on by potassium, the results were still more extraordinary.

In the compound obtained by the distillation of sulphur in oxymuriatic gas, muriatic acid is conceived to be in its dry state.

When potassium was acted on by this compound, a slight effervescence is first produced; and if the potassium is in larger quantity than the liquid, it soon explodes with a violent report, and a most intense light.

When large exhausted retorts were used in this experiment, they were shattered in pieces. One grain each of the substances, produced an explosion louder than a musket.

In an experiment when a quarter of a grain was used, a solid compound was formed of a deep gray tint, which, by a gentle heat, burnt with bright scintillations; and in water it inflamed,

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and gave most brilliant sparks. These effects may probably be connected with the decomposition of the acid.

The only effects produced by galvanic electricity on the fluid compounds, containing dry muriatic acid, seemed to be the development of its constituents in the gaseous form.

The experiments detailed afford some new views relative to the nature of acidity.

The generalization of Lavoisier, except in cases which are still under investigation, and concerning which, no precise conclusions can as yet be drawn, happily applies to the explanation of all the new phenomena.

Observations.—This paper is the history of laborious and successful investigations on some of the most abstruse chemical subjects. We shall not insult our readers by pointing out its merits, which are self-evident; they depend on facts which speak for themselves, and are their own eulogium; the absence of speculation too, when so many alluring subjects presented themselves, is very much to the honour of the author.

Of all the experiments, that of the action of potassium on ammonia, is perhaps the most curious and important. It would be presumption in us to draw any conclusions from it, when Mr. Davy himself, who has so often repeated and varied the experiment, and seen it in its different stages and under different circumstances, is undetermined, advancing the inferences he has ingeniously made in the modest and truly philosophical form of queries. We impatiently wait for new information on the subject, and advise our readers to form no precipitate conclusions; for the problem concerning the nature of nitrogen is not to be solved by calculations or reasoning, but by new facts. If the French chemists had been guided by the same cautious spirit as Mr. Davy, they would have subjected the experiment to a more rigorous examination, before they had made conclusions which affect the whole theory of chemistry, and which, if received, must produce a new revolution, the re-establishment of the exploded doctrines of Stahl, and the overthrow of the received theory of Lavoisier. But at present, there is little danger that this change will take place; for Mr. Davy has already confuted the erroneous reasonings of the French inquirers, whose views of this subject tend rather to confuse, than to enlighten science.

The analyses of the different inflammable substances lead us one step farther into the knowledge of their component parts. Hydrogen had been suspected to exist in sulphur. These new researches prove that it is a constituent part, probably an essential one, both in sulphur and phosphorus. They render it probable likewise, that the diamond, contrary to the received opinion of its simple nature, is a compound containing oxygen.

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The results of the experiments on the boracic acid confirm former analogies, and render it extremely probable that this substance has a metallic basis. Mr. Davy has certainly procured from it an inflammable matter bearing the same relations to boracic acid that sulphur and phosphorus bear to sulphuric and phosphoric acid. The decomposition of this acid, but in an imperfect manner, has been likewise effected by the French chemists, MM. Gay Lussac and Thenard; but the priority of discovery belongs certainly to Mr. Davy, who ascertained, when engaged on the decomposition of the alkalies in 1807, that when boracic acid was acted on by Voltaic electricity, inflammable matter was separated from it at the negative pole, and oxygen at the positive. In the spring following he decomposed it likewise by potassium, and it is this his instrument with which the French chemists have worked.

The new views presented us on the nature of acidity, amply repay the labours made in the decomposition of muriatic acid. Muriatic acid is indeed as really decompounded in these experiments, as sulphuric acid is when separated into sulphur and oxygen: in the latter case the sulphur is not simple; in the former, one of the component parts is abstracted, namely, water. The discovery of the real difference between muriatic and oxymuriatic acid gas, will explain, in a clear and beautiful manner, those anomalies which have never been satisfactorily removed, though attempted by the most ingenious chemists. It can no longer with propriety be said (even were the natures of the alkalies unknown) that oxygen is the general principle of acidity; for we find there are substances which, reasoning from analogy, should be considered as intensely acid, yet when put to the test, prove to possess no acid properties whatever, and which acquire them only by the addition of water.

On the Production of Metalloids from Potash and Soda, by means of the negative Pole of Volta's Pile. Drawn up by Mr. Weiss, *from Mr.* Ritter's *Letters to him.—Journ. de Phys. March* 1808.

The news of Mr. Davy's discoveries was received at Munich by Mr. Soemmering, 12th December 1807, and the Academy of Sciences there ordered Messrs. Imhoff and Ritter to repeat the experiments. The present communication is an abstract of what Mr. Ritter communicated to Mr. Weiss, of Leipzig, who was at Paris, by several letters in February 1808.

Mr. Ritter's piles were composed of from 50 to 400 pairs of plates formed of 3 parts of zinc alloyed with one of tin, separated by pasteboard moistened with a solution of muriate of ammonia: each plate acted by a surface of $1\frac{1}{2}$ square inches. Once he used

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a pile of 50 pairs of dishes, acting by from 36 to 40 square inches, filled with sulphuric acid, diluted with 30 times its weight of water. The action of this pile, when the acid was diluted with 40 times its weight of water, is equal to that of 50 pairs of plates of 36 square inches, each acted upon by a solution of muriate of ammonia: when the acid was diluted with 30 parts of water, the dish pile is the strongest; and if only 15 parts of water are employed, the dish pile is far stronger than the other, although the electric tension is always inferior to common piles of an equal number of pairs of plates.

A pile of 300 pairs of small plates alternating with a solution of muriate of soda, is more advantageous in decomposing potash and soda, than the apparatus composed of 50 large dishes filled with sulphuric acid; although the last yielded sparks, and were more efficacious in producing combustion than the former; so that no advantage will be gained by using large plates, unless the number of the layers is also augmented.

The mere production of the new metalloids may be observed even by a pile of 100 pairs of small plates; and if the observer is accustomed to these kind of operations, a smaller number will suffice; and a common needle, or any similar instrument, may be used for the negative wire. Neither is it necessary, as is hereafter fully shewn, to use pure alkalies.

It is a very unfavourable circumstance, that the alkalies are not only very imperfect conductors, but they also belong to the class called by Mr. Erman unipolar negative conductors. As, in repeating Davy's experiments, it is only necessary to attend to the negative side, the positive pole may be rendered a better conductor by being surrounded with a sufficient quantity of water, or still better by plunging the positive end of the piece of alkali that is employed into a saturated solution of the same alkali, in which the positive wire is to be inserted. By this means an unipolar positive passage is produced, and a singular transition is thus produced of one unipolar passage into another; a case that Erman has not noticed. The passage might even be made bipolar by plunging the two wires in the same saturated solution of the alkalies, but then the metalloids would not be obtained.

The best way of moistening the alkali is to wet the surface with pure water, and leave it for some time to diffuse itself through the alkali, repeating this operation until the proper degree of humidity is obtained.

As the alkali is heated by the action of the pile, and this heat destroys the metalloids nearly as fast as they are produced, and as a very low temperature is equally disadvantageous, the temperature of our ordinary apartments seems the most proper for this operation: but the metalloid from soda bears more heat than that from potash.

Operators, who are not expert, may sometimes obtain these

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bodies without knowing them, and suppose that they are mere bubbles of gas, covered with a whitish crust; but on separating this crust with the point of a needle, the metalloid is rendered visible.

These bodies are more silvery than quicksilver, and have also, especially that produced from very moist potash, rather a greasy lustre; but this is probably owing to a thin film of potash which surrounds the globules.

The metalloids are more easily reduced into small globules than quicksilver; that from soda has more adhesion than the one from potash.

The freezing point of the metalloids seems to be rather higher than is stated by Davy. If a low temperature is employed, both of them may be obtained at first in a solid state, and even in an arborescent form; but these dendrites soon melt and run down into globules.

They conduct electricity very perfectly, provided the globules are completely contiguous, which may easily be managed, by placing the two wires very near each other upon the potash, and gradually retiring them to a greater distance. They exhibit the phenomena of electrified liquid metals in more perfection than the others, on account of their great divisibility. Each portion of quicksilver acquires two poles, which separate from one another, but afterwards unite again, and thus affords a continual change of motion, which Mr. Ritter compares to pulsation and muscular motion. This phenomenon is exhibited by the metalloids, and especially by that of soda; the globules very frequently turn round all ways, and those in communication with the negative wire yield gas, at which time, however, the globules remain at rest.

When the metalloid from potash is inflamed by water, it yields vapours which have no sensible scent; the flame has much resemblance to the red rays of strong galvanic sparks. More pain was produced by touching the tongue with the metalloid than with a much larger portion of caustic potash. It may also be set on fire by steam, the vapour of the breath, or even the invisible exhalations of water in our rooms: but it requires a heat nearly approaching to redness, to set it on fire in the dry way. Black spots are left upon the metal on which it was heated.

Brugnatelli (Giornale Italiano, 1808, No. 44) says that potash recovered from the metalloid by contact of air, does not yield the metalloid substance on being again exposed to the pile. Mr. Ritter, from some slight experiments, supposes that this opinion is not true.

No sensible decrepitation is produced by grinding the metalloids along with hyperoxymuriate of potash.

As soon as they were thrown into sulphuric ether, which, from its specific gravity, appeared to be good, a large quantity of gas

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was emitted for a few seconds, and solid potash was found at the bottom of the vessel: this potash afterwards became liquid. The ether having stood upon potash for some time, the metalloids, on being thrown into it, disengaged gas much slower, but for a longer time. The common ether of the shops produced a very violent emission of gas, but no flame.

The use of common olive oil is a good method of preserving these metalloids from the air, as they swim upon it: but naphtha is still more useful.

Although these bodies return to the state of alkalies by exposure to the air, the change probably depends upon the moisture of the atmosphere, and would not take place in perfectly dry air. Sometimes on presenting largish masses of them to the flame of a taper, and heating them nearly to redness, they have become white externally, but, on being thrown into water, have shewn that the internal part was not altered.

Soda yields less of this kind of substance than potash, and the metalloid from soda is less affected by the air, heat, or even water. In one hundred trials with water, light was emitted in only ten instances. When the metalloid from soda is indeed still in contact with the negative wire, it is more inflammable, but the light is always weaker than when the metalloid from potash is used.

To facilitate the procuring of these substances, it is proper to augment the surface of the negative pole as much as possible; but, in order to examine the progress of the operation, the negative wire cannot be made too fine at the point.

These substances are procurable not only from the purest potash or soda, but also from common caustic, ordinary potash, barilla, acetate of potash, carbonate of soda, or acetate of soda, and soap, if previously dried; even muriate of soda, or common salt, yielded traces of the metalloid from soda. In this latter instance, as also when common soda was used, the positive pole yielded oxymuriatic acid gas, which although formed quite close to the negative pole, did not hinder the production of the metalloid globules.

Even the combinations of the alkalies with the earths yielded traces of these substances. Leucite (white garnet) and natrolite soaked in water for some hours, and then wiped dry, being placed between the two wires of 400 pairs of plates, yielded the metalloid substance, but in such small globules that they could only be discerned by a lens.

Hitherto only the products of the negative pole have been examined; nevertheless, some singular appearances take place on the positive side. A light appears there, particularly if the alkali is not too moist, as soon as the communication of the two poles is completed, which sometimes lasts for several seconds; it then disappears, and reappears from time to time without any apparent cause. Sometimes this light is calmer, and resembles an

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aurora borealis. There are also sometimes sparks of the same colour observed at the instant of completing the communication, which seem to arise from the instantaneous combustion of the nascent metalloid; and, indeed, a thin slice of potash being suspended from the positive wire, burned like a leaf of metal, and emitted similar sparks.

There is also a peculiar smell observable at the positive pole, which appears to have some analogy with the odour of an excited electric, or that smell which is perceivable in the air during thunder-storms. This smell is very powerful, as near the apparatus it overpowered the scent of the muriate of ammonia, which was employed to form the pile, and had been in a state of decomposition for several days. If a plate is put between the two poles, not the least trace of this odour can be perceived on the negative side. Care must be taken that neither the alkali nor the wire can furnish any extraneous smell. These phenomena of the positive pole are certainly produced by some peculiar substance, as they are not observable unless an alkali is present.

The most probable conjecture on the nature of these metalloid substances is, that they are hydrogurets of potash or soda, notwithstanding Davy himself considers them as reduced or metallized alkalies. Brugnatelli also supposes the substance from potash to be a reduction of that alkali, but differs from Davy in classing it, not with metals, but with sulphur, phosphorus, carbon, &c.

Their lightness, and habits with water, are in favour of their being hydrogurets, and all the phenomena are easily explained upon this supposition; but as the experiments of Messrs. Gay Lussac and Thenard have been directed towards the determination of this question, the results of their inquiries are impatiently expected.

The quantity of hydrogen that enters into the composition of these hydrogurets is probably very small, as is the case with other analogous substances: this seems evident from the quantity of metalloid formed by a given portion of alkali, and the almost instantaneous production of the metalloid. The substance formed at the positive pole is probably an oxide or hyperoxide of the alkali, and has no analogy to the metals.

A singular experiment shewed that when the rays of the solar *spectrum* were concentrated by a lens of an inch in diameter, the focus formed of the violet rays produced on potash a decrepitation which announced that the metalloid was formed, while the focus formed of red rays produced an odour similar to that observed at the positive pole. The red rays produced the same odour on soda, but the violet rays did not produce any decrepitation with that substance.

The strongest effects, however, were produced by the invisible rays of the *spectrum*: these effects are very analogous to other

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known facts, especially as light acting upon slightly moistened muriate of silver does not produce the simple metal, but a hydroguret of it.

Observations.—Similar experiments, with much more ample details relative to the metals of potash and soda, have long since been presented to the public by Mr. Davy (see our fourth volume).

We cannot acquiesce in the methods recommended by Mr. Ritter to procure these metals. From Mr. Davy's experiments and our own, we conceive the best mode is to use the pure alkalies after they have been heated nearly to redness. In this state, by breathing a moment on their surface, they become conductors.

When potassium is wanted for experimental purposes, the fortunate method discovered by the French chemists will of course be adopted.

Contrary to Mr. Ritter's assertions, Mr. Davy has found that the metals of potash and soda cannot be preserved in olive-oil, as it contains a sufficient quantity of water and oxygen to reproduce the alkalies. And potassium detonates loudly with hyperoxymuriate of potash when struck with a hammer.

Mr. Ritter's theory of the composition of these metals appears to be analogous to that of the French chemists; but the recent investigations of Mr. Davy have shewn the fallacy of this hypothesis.

On the Union of Tan and Jelly. By John Bostock, *M. D.*—*Phil. Journ. No.* 106.

This may be considered as a continuation of his former papers on the analysis of animal fluids.

The characters of jelly are, solubility in water, formation of an insoluble compound with tan, and its solution in water being concreted by cold, and redissolved by the application of heat.

From the experiments of Biggin and Davy, and the express assertions of the latter, it was thought that tan and jelly formed an uniform compound: so that if the quantity of tan be known, and the weight of the precipitate, that of the jelly might be ascertained.

As the object of the present experiments was, to ascertain by means of tan the quantity of jelly in any animal fluid, it became necessary to find some substance in which the real quantity of tan might be known without any long calculation.

Galls were first examined, but no two parcels agree in quantity of tan that they contain. An infusion of one part of galls in eight parts of boiling water for two hours, is usually a transparent, deep brown liquid, containing one tenth of solid matter;

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but the infusion is sometimes thick and muddy, and cannot be rendered transparent by filtration or rest; its colour also passes into a bottle-green, and it sometimes contains only one fourteenth of solid matter. If this infusion is evaporated, the residuum, although it be yielded by a transparent infusion, is not perfectly resoluble in water, so that some change has taken place in the solid matter.

Hatchett's artificial tan certainly has a strong resemblance to the infusions of astringent vegetables; but although it affords a precipitate with jelly, yet the fluid remains muddy after the operation, and the precipitate cannot be completely separated from it. This circumstance appears to arise from the tan containing an excess of acid, as is indicated by Hatchett's pointing out its property of reddening litmus.

As to catechu, it is not only much more variable in its qualities than galls, but is never entirely soluble in water, some part being only suspended; and these infusions are rapidly altered by exposure to air, a portion of the catechu being deposited, partly upon the sides of the vessel, creeping up to some distance above the surface of the fluid. This deposit is lighter coloured, and less soluble in water than crude catechu. The precipitate by jelly does not subside entirely, nor can it be totally separated by filtration.

Extract of rhatany not only contains more tan than any other known substance, but this tan is also more homogeneous, more soluble in water, and precipitates jelly more readily and completely: it is in fact the best substance for these experiments.

Isinglass has been said by Hatchett and Davy to consist nearly of pure jelly; but most of it contains insoluble matter in various proportions: in one case the insoluble part was one twentieth of the mass; and this can only be got rid of by boiling the isinglass in water, and evaporating the jelly to dryness.

Glue is, indeed, entirely soluble in water; but it contains some albumen, as it yields a precipitate on being boiled with oxymuriate of quicksilver. The muriate of soda that glue contains may also have some effect on the combination of jelly and tan. Glue, as usually prepared, contains a considerable proportion of water; for on being dried in a heat of 150° for twenty-four hours, it lost 10½ per cent. of its weight. Glue differs considerably from isinglass in some respects: a solution of glue containing one twenty-fifth of solid matter, although strongly adhesive, was quite fluid when cold; a similar solution of isinglass would have been perfectly concrete. When glue was soaked in water for forty-eight hours, it was so far dissolved that the upper part of the fluid precipitated an infusion of galls; isinglass in similar circumstances increased in bulk, but the fluid was scarcely affected by tan.

When the tan and jelly are not employed in a state of considerable concentration, and not put together in the proportion that forms the most perfect compound, the weight of the preci-

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cipitate cannot be ascertained, on account of the precipitate separating very slowly, or not at all, from the fluid, and adhering to a filter, so that it cannot be separated. Even the infusion of rhatany, or solution of isinglass, on being passed through a filtre, augmented its weight very considerably; and therefore, except when the fluids neutralize each other so as to be totally precipitated, the quantity of the precipitate cannot be ascertained, from the weight gained by the filtre.

It is also to be observed, that the substance formed by the union of tan and jelly varies considerably according to circumstances, and in no two instances have the results exactly coincided.

Extract of rhatany dissolved in ten times its weight of water, was added in three separate portions to a similar solution of isinglass, kept liquid by heat; copious precipitates fell down, and after some time the supernatant fluids became clear, and the separated precipitates were exposed to the same degree of heat. All the residual fluids precipitated jelly, so that they still contained uncombined tan.

Ten parts of tan with eight of jelly, yielded sixteen of precipitate, in which the constituents were nearly in an equal proportion. This precipitate was dark red, hard and brittle, with a shining fracture. Water boiled on it was not the least affected by jelly.

Ten of tan with 4 of jelly yielded 9.5 of precipitate, in which the constituents were in the proportion of 58 to 42. This precipitate had a brown tinge, and was hard but rather tough.

Ten of tan with 2 of jelly yielded 7 of precipitate, in which the constituents were in the proportion of 71.5 to 28.5. This precipitate was of a bright reddish brown, and could be pulverized between the fingers. On boiling this precipitate with a large quantity of water, the water acquired a reddish tinge, copiously precipitated jelly, and was slightly affected by the addition of iron, as it contained a minute portion of gallic acid; but muriate of tin produced no effect upon it. The boiled precipitate approached in its appearance to the first, but was deeper in colour and harder.

It is therefore evident that neither the quantity of tan or of jelly in any fluid can be ascertained with any degree of accuracy by this process; but this is of slight importance in animal analysis, as, contrary to the common opinion, it does not appear that any jelly is to be found in any animal fluids; at least none is to be found in the blood, albumen ovi, saliva, fluid of the hydrocephalus, of spina bifida, or of ascites, nor in the liquor amnii. By far the largest proportion of animal matter in all these fluids is albumen, either in its coagulated or uncoagulated state; and beside this, another animal substance is found in them, to which the

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name of mucus has been given; but whether properly or not, must be the subject of future consideration.

Observations.—These ingenious experiments, though they have not effected what Dr. Bostock had in view, have increased our knowledge, and given us more definite ideas on the subject. This paper may be considered as forming part of an interesting series on the analysis of animal substances, published in our second volume, page 487, and which may be consulted with much advantage by those who are engaged in similar pursuits.

On the Production of an Acid and an Alkali from pure Water by Galvanism. By Mr. CHARLES SYLVESTER.—*Phil. Journ. No.* 104.

ALL the experiments in which Mr. Davy has produced the apparent base of an alkali, an earth, or even an acid, are nothing more than degrees of the same process, as all the bodies he has operated upon are oxides of hydrogen. The acid and alkali can be produced from pure water in such abundance, that they must be derived either from the water or the apparatus.

The importance of electrical agency in chemical processes consists in furnishing hydrogen and oxygen in a pure and nascent state; for in all decompositions of the alkalies and earths, water has always been essentially necessary to the change. Water with different proportions of oxygen forms acids, and with hydrogen, alkalies, earths, and metals. When water is galvanized in two portions, separated by a moist conductor, an acid is produced on the side that the nascent oxygen is presented to, and an alkali is formed by a similar fixation of nascent hydrogen. In the pretended decomposition of this alkali, it combines with an extra proportion of hydrogen, and forms a metal; and when this metal is said to form an alloy with another metal produced from ammonia, it is really only combined with a greater proportion of hydrogen.

There seem to be only two kinds of simple matter, one resembling oxygen possessing negative electricity in the highest degree; the other an inflammable substance of the nature of hydrogen, and positive. Both these bodies are permanently elastic; but equal portions of them being combined together, would produce a body of the greatest possible density, on account of the attraction being at its height: as either of them predominates the attraction is diminished. Of course no solid body is simple.

Observations.—These hypothetical opinions of Mr. Sylvester concerning the production of all chemical substances from hydrogen and oxygen, are borrowed from Girtanner. In this en-

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lightened age experiment is our only guide, and it will be unnecessary to make any observations on these remarks of Mr. Sylvester.

On the Sulphuret of Lead. By Mr. DESCOSTILS.—*Memoires d'Arcueil, Tom.* ii. 1809.

As the action of gases on metallic sulphurets has been examined hitherto by no chemist, excepting indeed that of atmospheric air aided by a high temperature, by MM. Guenyveau and Gay Lussac, Mr. D. conceived that the results of experiments which had this object in view would throw great light on all metallurgical operations; for this purpose he selected the sulphuret of lead.

The Action of Heat.

If sulphuret of lead is intensely heated in a luted retort, connected by a glass tube with a water pneumatic apparatus, a little sulphurous acid gas, formed by means of the common air in the vessel, first appears; small drops of sulphur are next seen in the interior of the glass tube; if, when they cease to be formed, the retort is broken, the neck of it is found lined with a sublimate of sulphuret of lead, partly crystallized; and a subsulphuret remains, which is apparently fused, is brilliant and lamellar, and ductile in a slight degree.

The Action of the sulphurous and carbonic Acid Gases.

The sulphuret in small pieces was introduced into a porcelain tube, the ends of which exceeded the furnace by 1½ or 2 inches; one extremity was connected with a water apparatus; the other admitted the gases, which were sent through the tube when it had acquired a cherry-red heat.

Neither the sulphurous nor carbonic acid gas decomposed the sulphuret of lead; the only effect was, its sublimation and formation of imperfect crystals.

The Action of the Vapour of Water.

The vapour of water causes the sublimation of a great quantity of the sulphuret; the residue is sulphuret of lead unaltered, metallic lead, and oxide of lead; during the operation, hydrogen and sulphurous acid are disengaged.

The Action of Hydrogen.

Hydrogen carries over with it sulphuret of lead; but the principal part of the sulphuret is brought by the abstraction of the sulphur by the hydrogen to form sulphuretted hydrogen into the state of ductile lead. But this effect only takes place at a certain

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temperature, in that part of the tube which lies immediately over the furnace.

This decomposition explains the origin of the sulphuretted hydrogen produced by galena worked in the open air, by means of wood or moist charcoal. The Dutch chemists have proved that sulphur decomposes at a high temperature carburetted hydrogen, and forms sulphuretted hydrogen.

The Action of atmospheric Air.

Mr. Guenyveau has found that the sulphuret of lead is converted almost entirely by the slow action of air and heat into a sulphate; but if the current of air is passed rapidly over the ore heated to redness, white dense vapours are produced which have a strong smell of sulphurous acid gas, and which deposit on whatever they come in contact with, a fine white sulphate of lead, the same as is seen to volatilize in the furnace. When the fumes have ceased, a quantity of pure lead is found in the tube, equal nearly to half the weight of galena employed.

The production of the sulphate of lead is to be attributed to the action of the oxygen of the air on the molecules of the sulphuret separated from the mass; the nitrogen and sulphurous acid gas favour probably the volatilization of the galena.

The Action of a Mixture of atmospheric Air and sulphurous Acid Gas.

As sulphurous acid gas causes great part of the galena to rise in vapour, the quantity of sulphate produced is much greater than in the last experiment; the results otherwise are the same.

From the preceding experiments it appears evident, that the sublimation of sulphuret of lead is singularly favoured by a current of any gas, which acts by means of its chemical properties.

Lead, it appears, is not near so volatile as its sulphuret. The sulphate is unaffected by the gases, if those which exert a chemical action on it are excepted. Hydrogen, for instance, decomposes it.

It is clear, from the property gases have of aiding the sublimation of the ore, how great the loss must be from their action, and how disadvantageous it must be to separate the sulphur in the state of sulphurous acid gas. In the reverberatory furnace, for instance, employed for roasting the ore, whether sulphurous acid gas is produced by the direct action of atmospheric air on the sulphur, or by the reaction of the sulphate on the sulphuret, the chimnies in a short time are full of sulphate of lead, which is only a small part of what rises in vapour.

In the wind furnace the loss should be, and in fact is, much more considerable; for the heat is more intense, and the blast acts both on the sulphur and lead; where this furnace is used many thousand quintals of lead are annually dissipated in the at-

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mosphere. The scoria which is mixed with the roasted ore may impede the sublimation in some degree; but if the sulphate abounds, or the ore is badly roasted, much subsulphuret is formed, the reduction of which is attended with a new loss.

We must conclude from the preceding experiments, that the most profitable way of reducing the sulphuret, is to decompose it by iron, or any other substance that absorbs the sulphur without affording any gaseous product. But it is necessary, before the decomposition takes place, to prevent the contact of the blast on the mixture. The scoriæ may be added with less inconvenience than when iron is not used, as the iron absorbs the sulphur in the midst of the fused mixture. It is, however, impossible to shelter the galena entirely from the blast, and there is a considerable loss, as may be seen by the following experiments.

If a mixture of six parts of sulphuret of lead, and three of iron filings, is heated in a retort placed in a similar manner to that in which galena was submitted to the action of heat alone, little of the ore will rise in vapour, if the retort remains sound; but if it cracks, the sulphuret is disengaged in black fumes, as the extremity of the tube is already too cold to convert it into a sulphate.

To avoid the inconveniences attending the last method, a reverberatory furnace may be used, and the mixture covered with a thin layer of an easily vitrifiable earth. In this way little loss is to be feared.

In many places the high price of iron prevents its use; to suppy its place successfully is almost impossible, for none of its advantages can be expected from its oxides, either alone or mixed with charcoal-powder, as they necessarily generate much gas. To obviate this difficulty, Mr. Descostils proposes, with diffidence, the reduction of the oxide of iron to its metallic state, by exposing the ore broken in small pieces to a red heat for some hours, with charcoal-powder.

Observations.—The results of these experiments, though not of general importance to chemistry, are well deserving of the attention of metallurgists, for they explain some phenomena which have been erroneously considered, and they are likely to lead to a more economical method for the reduction of lead than that in present use.

On a new Acid, obtained from Ginger.—Phil. Journ. No. 105.

Ginger was infused for a few days in six times its weight of nitrous acid, and then rather more than an equal quantity of water was added, and kept at a boiling heat, adding water from time to time, until the nitrous smell went off. The solution was saturated with carbonate of lead, and filtered; after which the

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lead was separated by sulphuric acid, and the liquor again filtered and evaporated, when it left an acid resembling in appearance short white pieces of raw silk.

This acid oxidizes zinc and iron; it dissolves potash, soda, ammonia, barytes, strontian, lime, magnesia, and the oxides of zinc, iron, lead, and copper. It forms a soluble salt with barytes; remains in the solid form at 212°; is more soluble in water than the benzoic or suberic acid; and does not precipitate copper of a brown colour. The taste of the (perhaps super-) zingiberate of magnesia, is intermediate between that of acetite of lead and triple supersulphate of alumine.

On sulphuric Ether, and its Preparation. By Mr. Boullay.—*Ann. de Chim. No.* 186.

In the usual mode of preparing sulphuric ether, the whole of the product is not equally pure, as the last portions retain a disagreeable odour, even after repeated rectifications.

Fourcroy and Vauquelin have shewn that alkohol is transformed into ether by the attraction of the sulphuric acid for water; and this change precedes all the other effects of that acid; so that as soon as any other product passes over, we may be certain that the formation of ether no longer takes place. It seems, therefore, probable, that the addition of alkohol during the distillation, so that it might never be less than one third of the liquid in the retort, would hinder the destruction of the sulphuric acid; and the ether would continue to be produced until the acid was so diluted as not to act with sufficient force on the alkali.

Ten chiliogrammes (20 lbs.) of sulphuric acid, at 66° of the hydrometer, were poured into a tubulated glass retort, placed on a sand bath, connected by a glass serpentine to a large bottle, and then by a syphon to a second bottle filled with water. The same weight of alkohol at 36° Beaume's hydrom. was introduced by a tritorium with two glass cocks (Retrospect, iv. 66), the tube of which descending nearly to the bottom of the retort, obliged the alkohol to traverse the acid; the quicker this is done, the less is the mixture coloured; as soon as two chiliogrammes passed over, another 10 chiliogrammes of alkohol at 40° was gradually introduced as fast as the ether distilled over. Fifteen chiliogrammes of a clear ethereal liquor, without any sulphurous scent, were obtained, from whence 8 chiliogrammes of pure ether were procured by rectification.

The alkohol added during the distillation is taken in a purer state than that used at first, on account of the acid being weakened by the water abstracted from the former portion.

The residuum left in the retort is of the colour of beer, and very clear; when heated it becomes black, sulphureous, and oily;

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and might be used to make Hoffman's mineral anodyne liquor. Or it may be used as sulphuric acid in cases when the presence of alkohol will not be hurtful, as in forming salts with it.

Observations.—We profess ourselves decisively hostile to the introduction of complicated apparatus, however elegant or commodious, into chemistry, being fully of opinion that the same end may, in almost every case, be obtained by simple means.

The addition of alkohol during the distillation of ether is certainly an improvement, and the principle might probably be extended to other operations; but it seems sufficient to use a bent tube dilated at the upper end, so as to form a funnel, inserted either into the opening in the arch of the retort, or passing into the mouth of the retort, between it and the receiver.

NATURAL PHILOSOPHY, ARTS, AND MANUFACTURES.

Invention of a Telegraph. By Major CHARLES LE HARDY.—*Trans. Soc. of Arts, Vol. XXVI.*

THIS telegraph, which is intended to express numbers, to which words may be referred at pleasure, consists of nine bars or radii, answering to the nine digits; and of four polygonal or concentrical bars, which intersect the radii, and beginning with the outside one, represent units, tens, hundreds, and thousands. Over each of these bars an index traverses which marks the number of thousands, of hundreds, of tens, and of units, as far as ten thousand. If it were required to represent the number 9202, the index on the circle of thousands must be moved to the radius 9; that on the circle of hundreds to the radius 2; and as there are no tens, the index on the circle of units must be placed on the radius 2; and these combined will represent the required number. But as ten thousand is not sufficient to express all the words in the English language, two square boards are added in the corners, one of which represents 10,000, and the other 20,000; and when both are exhibited together they denote 30,000, which added to the number that the other part of the apparatus is capable of expressing, affords a ready means of representing any number not exceeding 40,000, which is many thousands more than there are words in the English language.

The signals are worked by means of a wheel and rack, put in motion by a winch; except the two at the corners, which are fixed on iron bars, and made to revolve a quarter round by levers connected with cross bars, by means of ropes or wires. To prevent the necessity of inspecting the signals, an index which re-

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volves with the signal-board points out the number on a dial-plate that it designates; so that the telegraph may be worked correctly within doors. The signal-boards are secured at any particular place by means of a bolt adapted to the purpose.

Major Le Hardy remarks that the number may easily be carried to a much greater extent than 40,000 if necessary, but he conceives that this is fully sufficient for every purpose. All the words in Entick's Dictionary amount to about 25,000, which may be numbered in regular succession, and then they may be forwarded as fast as they can be looked for in the dictionary. Another advantage arising from the use of words in a telegraphic correspondence is, that those of the same meaning in different languages have the same number; and hence the correspondence from one language to another will be sufficiently intelligible, though not grammatically correct. Proper names must be spelt, each letter having a corresponding number. The use of telegraphs has to this time been confined to military purposes; but it is here suggested that they might be used with great advantage in commercial concerns; that they might be erected at a trifling expense, and a correspondence carried on with them at the distance of five or six miles. The inventor made the experiment with one of eight feet by ten, and took down every number at the distance of a mile and a half, with the assistance of a telescope.

Observations.—We apprehend that some contrivance which to a certain extent answered the end of the interesting and important machine universally known by the appellation of *telegraph*, and which is of modern application and improvement, was known to the ancients at a much earlier period than many of our readers may have conceived. And when it is considered that the object proposed is, to obtain an intelligible figurative language which may be distinguished at a distance, and by which the obvious delay in the dispatch of orders or information by messenger may be avoided; and that the nature of this work requires us to present all the useful and interesting information in our power relative to each of the topics which come under our review, we presume that a brief historical sketch of that instrument will not be unacceptable to many of our readers.

As the name is descriptive of a contrivance by which information is almost instantaneously conveyed to a considerable distance, whenever we find a circumstance of this nature recorded, it is natural to conclude that it was effected by some agent possessing similar qualities. It is generally regarded as a well-authenticated fact, that the burning of Troy was known in Greece a very short time after it happened, and long before it was possible for any person to have arrived there from the scene of conflagration. One of the Greek plays opens with a scene in which a watchman

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descends from a tower in Greece with the news that Troy was taken. He thus expresses himself: "I have been looking out these ten years to see when that would happen, and this night it is done:" a proof that the ancients possessed some mode of conveying information very quickly to a great distance; but what that mode was, it is not so easy to determine. When the Chinese courtiers travel to distant parts of the empire, signals are made by means of fire from one day's journey to another, in order that preparations may be made for their reception. In most barbarous nations it was customary to give the alarm of war by fires lighted on the tops of hills; and Polybius styles the instruments used by the ancients for communicating intelligence in this manner, *pyrsæ*, because the signals were made by means of fire. A new method of accomplishing this object was invented by Cleoxenus (some say by Democritus), and much improved by Polybius. He divided the Greek alphabet into five parts, and expressed the letters on boards in five columns. The order in which these letters were to be taken was signified by torches held up in certain positions, previously agreed upon by the parties.

Neither this last-mentioned method, nor any other known to the ancients, however, appears to have been carried into general use; nor have we any account of any modern invention for the same purpose previous to the Marquis of Worcester's, contained in his "*Century of Inventions*," which was published in 1663. In this work he affirms that he had discovered "a method by which, at a window, as far as the eye can discover black from white, a man may hold discourse with his correspondent, without noise made or notice taken; being according to occasion given, or means afforded, *ex re nata*, and no need of provision before-hand; though much better if foreseen, and course taken by mutual consent of parties." This means of correspondence he also asserts to have been rendered so perfect that it could be put in practice "by night as well as by day, though as dark as pitch is black." On the 21st of May 1684, Dr. Hooke, so much celebrated for his mechanical genius, delivered a discourse to the Royal Society on a method of communicating one's mind at a great distance. In this discourse he asserted the possibility of conveying intelligence from one place to another at the distance of 30, 40, 100, 120, &c. miles, "in as short a time almost as a man can write what he would have sent;" and laid down directions for accomplishing it. The whole of this ingenious paper was published in Derham's Collection of Dr. Hooke's "*Experiments and Observations*," and from which it appears that his contrivance was little inferior to several that have been lately proposed for the same purpose. William Amontons, an ingenious French philosopher, about the year 1702, likewise invented a method of conveying information from one place to another by signals; and it is probable without any knowledge of what had

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been previously advanced on the same subject by the Marquis of Worcester and Dr. Hooke, as his method seems to be much inferior to that pointed out by this last gentleman.

None of these inventions, however, were applied to purposes of utility before the French revolution, when M. Chappe, towards the end of 1793, invented an instrument of a similar nature, and applied to it the name of *telegraph*: whether or not he knew any thing of the inventions of Dr. Hooke and Amontons it is impossible to say. The first chain of telegraphs formed a communication between the Louvre at Paris, and Lisle, a distance of about thirty-five leagues, near which town the French army then was. The first description of this instrument is said to have been brought from Paris to Frankfort on the Maine by a former member of the parliament of Bourdeaux, who had seen that which was erected at the Mountain of Belville, near Paris. Two working models of it were made at Frankfort, and sent by Mr. W. Playfair to the Duke of York: and hence the plan and alphabet of the machine came into England. M. Chappe's contrivance consisted chiefly of an upright post bearing a transverse bar about 10 or 12 feet in length, and 9 inches broad, moveable about a pivot in its centre; to each extremity of this bar an arm of about 3 feet long was attached by a moveable joint: by means of these three the combination of movement was very extensive, simple, and easy to be performed. A number of arbitrary positions were fixed to denote the letters of the alphabet; and these being always made precisely in the same manner by means of the mechanism employed to work the arms, intelligence was conveyed with astonishing accuracy and rapidity.

In consequence of this information various experiments were made in this country, and a communication by means of a chain of signals was soon established from the Admiralty-office to the sea-coast. The telegraph adopted for this purpose consisted of six octagonal boards, each moveable about an axis, and capable of being placed either vertically or horizontally, so as to be either visible or invisible at the nearest station at pleasure. These six boards are capable of exhibiting thirty-six changes by the most simple and easy mode of working them; and by these the letters of the alphabet and the numeral cyphers are denoted. This telegraph, however, was by no means considered as being free from objections and inconveniences, and various contrivances for remedying these have been invented since that period. To give an account of all these would greatly exceed our limits; we shall, therefore, content ourselves with mentioning a few of the principal of them. Mr. John Garnet proposed a very simple and ingenious instrument, which consisted merely of a bar or plank, moveable about a centre, round which a circle was described, having the letters of the alphabet and figures depicted round its circumference. An index which corresponded with the bar en-

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abled the operator to set it to any letter or figure he wished to communicate. A wire was fixed across the tube of the telescope used by the observer, which was to be brought to coincide with, or to be parallel to the bar; then the letter or figure to be communicated was denoted by a small index at the eye end of the telescope, which contained a circle similar to that encompassing the centre of motion of the bar. Another ingenious improvement was proposed in the Gentleman's Magazine: it consisted of a semicircle of twelve feet radius: this was to be properly elevated, divided into 24 equal parts, and each of these perforated with a circular opening of six inches in diameter. These apertures beginning from the left, denoted the letters of the alphabet, except K, J, V, X, and Q, which were omitted: the remainder of the holes were reserved for signals. The communications to be made in the day were pointed out by means of an index, and in the night by lamps. None of the preceding telegraphs, however, appear to exceed, either in simplicity, cheapness, or facility of working, that described by J. N. Esq. in vol. i. of the Repertory of Arts, Old Series. For a nocturnal telegraph it consists of four large patent reflectors, situated in the same plane and parallel to the horizon, but sufficiently elevated above it. Each of these reflectors is susceptible of being elevated or depressed at pleasure by means of winches, and the letters of the alphabet are denoted by the changes. When this telegraph is used in the day, gilt balls, or any other conspicuous objects, are to be used instead of the reflectors. Captain Pasley, of the Royal Engineers, also invented two telegraphs, one of them to be used by day, and the other by night:—for a description of these see our fourth volume, page 6.

Two methods have generally been adopted in communicating telegraphic information: in the one, each signal represents a single letter; in the other, a complete sentence. This last has generally been thought to possess an advantage over the other in point of expedition, but it is only partial in its application; and we have already stated it as our opinion, that the general elements of language only should be transferred, and that every real improvement in the telegraphic art must tend to facilitate the execution of this object. Our readers will readily perceive that Major Le Hardy's invention has a more direct tendency towards the accomplishment of this end than any other which has yet been laid before the public: its mechanism is simple, and its application universal. If any delay during the working be experienced, we conceive it will arise from the time which will be necessary to look out the numbers answering to the words at the first, and the words corresponding to the numbers at the last station; but a little experience, and the assistance of projecting labels fixed to the dictionary at the termination of every thousand, will completely remove this inconvenience, should it be found to exist, and

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under the rapidity of this mode of communication truly astonishing. Mr. Le Hardy obtained the silver medal of the Society for his invention.

Description of a Telegraph, invented by Chevalier A. N. EDELCRANTZ.—*Trans. Soc. of Arts, Vol. XXVI.*

THIS telegraph, or that part of the machine by which the signals are made, is composed of three vertical rows of flat vanes, or indexes, similar to those used in the English telegraphs. The middle row contains four, and each of the others, three of these vanes, or shutters, the fourth in the middle row being above the rest. Each index has a crank fixed to it, from which a wire, or rope, comes down to the machine, by which it is worked. The principal part of this machine is a barrel with a winch handle, by the revolution of which a bar of wood having an iron plate fixed to the top of it, is drawn down, and then wires, or strings, connected with the signal-boards being attached to this at pleasure, the signals are brought into an upright position. When the wires, or strings, are relieved from their connexion with the plate, a weight suspended from each signal-board brings it to its original position. The business of connecting these strings with the plate, is facilitated by means of ten keys, each marked with a figure denoting the signal-board, the string of which is opposite to it, and which it causes to slide into its proper notch in the plate, when required.

In working this telegraph, six signals are usually made in a minute, and 1024 changes, or signals, may be clearly shewn. A much greater number might also be exhibited, but it would require an attention to too many circumstances. Words which frequently occur in correspondence are represented by such signals as are most easily recollected. These are termed fixed signals, and may be readily known by examining the numbers of a table previously constructed, and referred to by the signals.

The great advantage of this contrivance is, that by the powerful leverage afforded by the winch and barrel, one man is enabled to move any number, or even the whole of the shutters at once, and to retain them in that situation, be the weather ever so boisterous, until the signal is answered; which is the more necessary in this telegraph, on account of its greater number of vanes, or indexes: but which cannot be done in the government telegraphs in England, even with their more limited number of vanes, without employing several persons at each telegraph. The inventor of this telegraph states that one man is sufficient to manage it at the two extreme stations, and two men at the intermediate ones.

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Observations.—We are inclined to regard Chevalier Edelcrantz's telegraph as being superior, in some respects, to those employed by the government of this country; but as considerably inferior in point of general utility to Major Le Hardy's. Each, however, received the same honorary reward from the Society to which they transmitted their improvements.

Method of raising large Stones out of the Earth. By Mr. Robert Richardson.—*Trans. Soc. of Arts, Vol. XXVI.*

The apparatus used by Mr. Richardson for this purpose, consists of a quadrangle and lifting tackle. The hind legs of the quadrangle are moveable at the top, and close in between the fore legs, for the convenience of carriage. The lifting tackle consists of fivefold blocks and rope, the former are about ten inches in diameter; a roller about seven inches diameter, which is turned by two long iron levers, and a winch handle, used as a safeguard and to regulate the power of the levers, and an iron plug which is fixed into the top of the stone and hung on to the hook at the bottom of the lower block by means of an oval iron ring. This plug constitutes the principal part of Mr. Richardson's invention, and consists of a cylindrical piece of iron about an inch, or an inch and a quarter in diameter. Its whole length is about nine inches; it is made broad in the middle, and has a hole for the ring to pass through, which connects it to the block.

The method of using this apparatus is, by first making a circular hole, about three or four inches deep, in the top of the stone to be raised, and driving the plug tight into it by means of a hammer applied to its upper extremity. The lifting tackle being then put in motion by three men, a stone of four or five tons weight may be raised in the space of ten minutes, and without first removing the earth from about it, or any other preparation except making the hole and fixing the tackle. Care, however, should be taken that it do not receive any lateral blow, as that immediately disengages the plug.

Mr. Richardson's account is accompanied by letters from two gentlemen who have used the apparatus, certifying to the full extent of the above-mentioned effects; and the Society voted him their silver medal for the invention.

Observations.—This singular fact appears to be well established by experiment; but the action of the plug is so contrary to what theory would induce us to expect, that we shall not attempt to theorise upon it at all. We hope it will be found to answer as well upon a more extensive trial, as it has done in a few experiments; and in that case, we doubt not that it will be found very

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useful in several of the northern districts, where the surface of the ground is partially covered with stones of this kind, as well as for any other purpose, where large stones are to be lifted to small heights.

Methods of equalizing the long and short Arcs of Vibration in Timekeepers. By Mr. WILLIAM HARDY, *of St. John Street, Clerkenwell.—Trans. Society of Arts, Vol. XXV.*

IN this communication, for which Mr. Hardy obtained the Society's silver medal, three different modes of accomplishing the same object are presented. The first of these is by a straight spring placed edgeways across the impelling pallet, and is screwed at the end opposite to the direction of the wheel, on its approach towards the centre of the pallet; the other extremity of this spring contains a flat or curved surface; on which the approaching tooth of the escape-wheel is received, which gives the impulse: the spring acts between two pins placed in the pallet near the end. The strength of this spring must be so adjusted, that it may yield a little to the force of the wheel in giving the impulse, and the different vibration will be made in the same time; the proper degree of strength, however, can only be ascertained by experiment. This method also possesses this advantage, that the surfaces in contact are less injured by the dropping of the wheel upon the spring than when it is received upon a solid substance; neither are the vibrations of the balance so much disturbed by the impulse.

The second method here proposed is effected by a straight spring screwed to the under part of the cock, placed on its edge diametrically over the cylindrical spring, and having a piece cut out to clear the balance arbor. One end of this spring is fixed to the end of the pendulum-spring, and the strength of the other is so reduced as to yield a little before this latter spring operates; on the opposite part of the cock where the spring is screwed, a stud is fixed projecting downward, and having a slit in it to admit a small piece at the end of the spring; each side of this slit contains an adjusting screw; their points are opposite each other, and so situated that the spring may move equally between them from its point of rest. The action of the spring between the adjusting screws must be less than the angle of escapement. "Let the balance be made to vibrate, so that the straight spring may move up to the adjusting screws upon each side, and no farther; being weaker than the pendulum-spring, its exertion will be less; hence the time of the vibrations will be prolonged; but as they increase, the exertion of the pendulum-spring will commence, and pro-

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gressively accelerate them; and this acceleration will always be in proportion as the exertion of the pendulum-spring is to the action of the straight spring between the two adjusting screws. Thus it will always counteract the accelerating effect of the escape-wheel in the small arcs of vibration, so that the whole of them shall be performed in the same time."

The same object is attained, in the third instance, by a piece of short spring-wire connected to the pendulum-spring by a small piece into which they are both turned about half a turn from the stud of the pendulum-spring. The other end of the wire is clamped, at its natural point of rest, to a sliding piece projecting from the stud of the pendulum-spring. Both springs will thus act together, and each will retain its natural point of rest; but when the sliding piece is a little moved, so as to cause each spring to exert a certain force in opposition to that of the other, both points of rest will be destroyed. "Thus by producing this counteracting force in the two springs at the lowest point of vibration, the accelerating effect of the escapement-wheel upon the balance in the small arcs of vibration will be corrected; thereby the whole of them will vibrate in equal time."

This paper is accompanied with an extract of a letter from Captain William Brown relative to a chronometer he had purchased of Mr. Hardy, in which he states that he could not discover that it had varied one second during his passage of twenty-four days from Liverpool to Gape De Verd, on the African coast.

Observations.—The means employed by Mr. Hardy for effecting the purpose specified in the title to this article appear to be simple and natural; and when adjusted with that degree of nicety which things of this nature require, will produce the desired effect to a considerable degree. This is strongly confirmed by the testimony of Captain Brown; as a chronometer which could be carried from this country to upwards of nine degrees within the torrid zone, with so little variation in its rate of going as he has stated, must be one of a superior construction. We, however, think it within the limits of possibility, that Captain B. might be destitute of the means of determining with such a great degree of accuracy. These methods appear to be only modifications of that which Mr. H. published in the 59th number of the Philosophical Journal, and we noticed at page 313 of our second volume. See also our remarks on this subject at page 309 of the same volume.

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Manufacture of Ropes and Sacking from Sheep's Wool, to answer the Purpose of similar Articles made from Hemp. By GEORGE WHITWORTH, *Esq.—Trans. Society of Arts, Vol. XXVI.*

MR. Whitworth thinks that woollen ropes and sacking might be substituted for those articles which are now manufactured from hemp, in many instances, with both individual and national advantage. Worsted ropes have been tried for agricultural purposes, and found fully to answer the expectation that was entertained respecting them. Traces and plough-lines, after having been in constant use for three months, did not appear worse than they were at first; and waggoners preferred ropes of this kind for binding loads of hay and corn, as those were carried home in a more regular and even state which were bound with them than others which were fastened with hempen ropes. They did not become stiff by being wet and dried again, as ropes made of hemp do. Mr. W. also thinks that their elasticity will render them proper for the purpose of breeching guns, and of being used in running rigging in particular situations. A rope of this kind was used in turning a cylinder from the axis of a water-wheel, and was found to wear longer than any one made of hemp had done in the same situation. A worsted rope 24 feet 9 inches long, and 3¼ inches in circumference, the weight of which was eight pounds three ounces, supported a weight of one ton thirteen hundred, three quarters and sixteen pounds, but was broken by the addition of fifty-two pounds more. The expense of ropes made from combed wool is stated to be about three-pence a pound more than those manufactured from hemp; but that this will be doubly compensated by the value of the old materials, which are capable of being manufactured again, and which are worth nearly two fifths of the original wool.

Sacking made from wool answers very well for bags used by farmers and corn-factors, and is very little dearer than hempen sacking. Mr. W. conceives that it might be used with advantage for the bottom of beds; and he states, on the authority of naval men, that it will make good hammocks. Estimating the raw materials at 84*l.* per ton, the following comparison of both their new and old value is given:

17 ounces hemp, worth, new, 13*d.* old 1*d.*
12 ounces wool, ditto 12*d.* old 4*d.*

With respect to the advantages to be derived from this substitute for hemp, Mr. W. observes: "Wool used upon my plan will furnish employment to manufacturers, who are now in great need; it will relieve the growers of long wool, by taking an

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article at a fair price, which must otherwise remain a drug upon their hands, and it will give new life and vigour to our internal commerce."

Observations.—In some cases the strength, in others the elasticity of a rope, is the most desirable property. In the former of these qualities, the kind of ropes proposed by Mr. Whitworth have been found to be far inferior to those manufactured from hemp. Mr. Chapman, in his treatise on "The Manufacture and Duration of Cordage," has stated an experiment which he made on a woollen rope; and from which he concludes, that its strength was only equal to one third of that of a good hempen rope of the same weight; and about two thirds of such white ropes as are usually made in the interior of the country. The circumference of this rope was 1.8 inches, and its length 24 inches, which increased to more than 37 before it broke, which it did when stretched by a weight of 7.7 cwts. when by the sudden reaction its length returned to 26½ inches. The experiment, however, shews the great elasticity of this kind of ropes, and therefore proves their fitness for purposes in which this quality is chiefly wanted. Sacking of this kind will probably answer for hammocks, provided it will bear painting as well as the cloth made of hemp, but not for the bottoms of beds in general. A light sail-cloth, we think, might be made of this kind of wool, which, from its elasticity, would be preferable for small vessels to that in use at present: and light woollen stuffs of this nature might also be used for a variety of common purposes for which they would, in some respects, be preferable to hempen or linen cloth now used. The quantity of hemp, which is annually manufactured into tarred cordage is the united kingdom, has been stated at 30,000 tons; and Mr. W. thinks that about 15,000 tons of long wool might be dedicated to this purpose. We, however, conceive that this quantity would interfere a great deal too much with our established woollen manufacture. The want of strength too in ropes of this kind will always constitute a material obstacle to their general adoption.

Improved Crank useful in working Telegraphs; by Mr. Manas Ward; *with Remarks on his Compensation Pendulum.—Trans. Society of Arts, Vol. XXVI.*

This improved crank consists of a wheel fixed on the axis of the signal-board, and having two segments of circles attached to it by means of a steel spring fastened to their adjoining ends, which causes them always to endeavour to recede from the circumference of the circle. The segments have grooves in their peripheries to

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contain two cords by which the signal-board is worked; and each has a notch in its exterior edge: these notches are a quarter of a circle, or 90° distant from each other. When the signal-board is placed in a vertical position, one of these notches is locked on a piece of iron by the force of the spring by which the segment is attached to the wheel, and it remains in this position until it is relieved by pulling the cord which passes over its periphery: this operation permits the wheel to revolve the contrary way, when a force is applied to the other cord; the other notch then catches the iron, and retains the signal-board in a horizontal position till it is released in a similar manner.

Mr. Ward's remarks on his compensation-pendulum relate to an error in the description of it, published in the last volume of the Society's Transactions, but make no alteration in the general description which we gave of this invention at page 454 of our fourth volume.

Observations.—The superior utility of this crank over those in common use, particularly in windy weather, is obvious; and its construction adds another proof of Mr. Ward's ingenuity to those already noticed in our second and fourth volumes. The Society to whom the model was sent rewarded Mr. W. with a sum of ten guineas for his invention.

Reply to Mr. Barlow's Article on floating Bodies. By G. Orr, *Esq.—Phil. Mag. No.* 134.

In this reply, Mr. Orr contends that bodies floating in a stream of water are descending an inclined plane, and are influenced in their progress by two causes, viz. the motion communicated to them by the motion of the fluid in which they float, and by their own weight arising from the inherent property of gravity. He also defends his "comparison between balls of different weights rolling down an inclined plane, and beams or barges of different weights floating down a running stream," by observing that the atmosphere is never perfectly at rest, except when all the particles of which it is composed are in equilibrio, which is seldom the case, and never generally so. Mr. Orr likewise objects to Mr. Barlow's position of a beam loaded at one end, moving parallel to itself when placed in running water; and affirms, that a "beam thus loaded cannot preserve its parallelism for an instant. It must obey the laws of gravity, and instantaneously begin to change its situation, the heavy end getting foremost."

Observations.—Such of our readers as are acquainted with subjects of this nature, and interested therein, will readily appreciate this reply of Mr. Orr's, and determine for themselves how far he

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has either defended his own positions, or invalidated Mr. Barlow's. For our parts, we do not conceive that he has rendered his original solution a whit more satisfactory than it was at first.

Suggestion for establishing a telegraphic Intercourse between London and Dublin. By the Rev. JAMES HALL.—*Phil. Mag. No.* 136.

Mr. Hall states, that on his arrival at Torr Point, Fair Head, Ireland, he hired a boat and sailed to Cantire in Scotland: the distance of which he found to be only eleven miles and a half, instead of between 20 and 30, as laid down in several modern maps. This suggested to him the idea of the proposed telegraphic communication, by which information might easily be conveyed from Fair Head to the Mull of Cantire; the distance between which is less than between two of the communicating points in the line from London to Portsmouth, which is nearly thirteen miles. Relative to this intercourse, he observes: "In erecting a telegraph southward from Dublin to Cork, and northward to Fair Head, there seems no difficulty at all. At Fair Head, and the opposite point in Scotland, signals to draw attention might be addressed both to the eye and the ear; those to the ear being calculated to rouse attention, and induce people to look out and apply to their glasses. From Cantire, through the islands of Arran and Bute, and other places, information might be sent by Glasgow, Edinburgh, &c. direct to London, and back again to Dublin, Cork, &c. in the course of a few minutes." A mode of communicating speedy information to our fleets in Cork harbour, as well as to them when in English ports, Mr. Hall regards as an object of great importance.

Observations.—In this communication Mr. Hall appears to have confounded Torr Point with Fair Head, or supposed them to be the same point of land, though the latter is situated several miles to the north-westward of the former; and considerably farther from the opposite coast of Scotland. In some of the best maps lately published, the least distance between Ireland and Scotland is laid down at very little more than Mr. H. has stated it to be. And with respect to the chain of telegraphs which he suggests, it certainly would not be void of utility; as a ready means of conveying intelligence between the metropolis and our shipping, at whatever port of the united kingdom they may be, may in many cases be a matter of the greatest moment; and therefore should be provided as far as circumstances will permit. We, however, apprehend that the conveyance of information through the extent of line which Mr. H. has pointed out, would occupy more than a few minutes.

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New Method proposed for measuring a Ship's Rate of Sailing. By Mr. JAMES BURNEY.—*Phil. Journ. No.* 106.

THIS new method consists in having a given length of line towing astern of the vessel the rate of sailing of which is to be determined, which will pull against her head-way: and as the ship's way increases, the pull of the line will increase, and the contrary. This method Mr. B. thinks is less liable to give erroneous or variable results than any which can be made near a ship; and, from a trial made in a boat, seemed to answer very well. The line being passed over a pulley at the stern of the ship, its inner end is to be fastened to a loose chain adapted to the purpose, or to have a number of small weights, made consecutive by short intervals of line attached to it. "In an improved state of the experiment, instead of using weights or a pulley, the inner end of the line (coming direct from the water) can be fastened to a spring, and communicate with an index that shall express the rate of sailing."

Observations.—How far this new method may be preferable, or whether or not it be at all preferable to either Mr. Boswell's, described in our vol. iii. page 360, or to General Grant's, inserted at p. 57, vol. iv. of this work, we shall not attempt to decide. It however is liable to the first, third, and fourth objections which we have urged against this last: and, in fact, the whole of what induced Mr. Burney to propose this new method seems to have been only guessed work, and therefore not at all to be depended upon.

On Deal Pendulum Rods. By Mr. E. WALKER.—*Phil. Mag. No.* 135.

IN order to give an appearance of importance to this communication, Mr. Walker asks, "who would believe, without having recourse to facts, that clocks with deal pendulum rods perform *nearly* as well as the transit clock at the Royal Observatory!" He then recapitulates the substance of his former paper on this subject, which we noticed in our 18th Number. He enters into some explanation relative to the making of the astronomical clock at Greenwich; and gives a table of the greatest variations in its daily rate of going; the mean annual variation of which for a period of six years was 3′ 9″, that of his clock he states at 5′ 41″; the difference he makes 1′ 51″; this, however, is 2′ 32″. Mr. W. has also subjoined an explanation of the hygro-

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meter, which he used to ascertain that this wooden pendulum varied in its length by different degrees of moisture: and this happens to be the door of his clock-case!

Observations.—This is another of Mr. Walker's *valuable* communications—we shall probably have to thank him for something *less* valuable in one of our subsequent Numbers.

On geometrical Proportion. By Wm. Marrat, *Esq.—Phil. Mag. No.* 134.

Mr. Marrat observes, that a great part of the obscurity with which this subject is enveloped, arises from the vague and ambiguous manner in which authors have defined the word proportion; and that it is frequently confounded with the words ratio, reason, analogy, &c. He also remarks, that the method of treating the subject geometrically, as in most modern books of geometry, is not the most eligible, or best adapted to learners; and that the difficulties attending the fifth book of Euclid's Elements are so great, that very few students ever thoroughly understand his demonstrations, chiefly owing to their inability to form a correct idea of his criterion of proportion. In order to avoid these difficulties, and exhibit the subject in a clearer and more simple light, Mr. M. defines Ratio to be the relation which subsists between *two* quantities with respect to their magnitudes. *Proportion,* he conceives to be an equality of ratios; and that the whole doctrine of proportion flows immediately from these obvious principles.

Agreeable to these premises, Mr. Marrat then exhibits and illustrates the chief propositions relating to the doctrine of proportional quantities; and for this purpose he makes use of the language of algebra, as being best adapted to the subject.

Observations.—The principles upon which Mr. Marrat has grounded his demonstrations are laid down with considerable clearness and precision; and the demonstrations are general, and applicable to all kinds of quantities both commensurable and incommensurable. The algebraic language too, in which they are expressed, from the rapidity with which it conveys the ideas of the operations supposed to be performed in these demonstrations, we regard as more suitable than any other to the subject.

One of the principal obstacles which students meet with in the fifth book of Euclid, is the difficulty of reconciling the idea of proportion which they have previously acquired, with the definition of it which is there given. Our first ideas on this subject are obtained by comparing together the magnitudes of external objects. These ideas are at first generally vague; but the study of

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arithmetic renders them much more correct; as by this we are taught to call four numbers proportionals, when the quotients arising from dividing the first by the second, and the third by the fourth, are equal to each other. And as this operation of division is as applicable to any magnitudes of the same kind as to numbers, the idea of proportion thus obtained becomes perfectly general.

On a Method of converting an ordinary Balance into a good hydrostatic Balance. By M. GIOVANNI FABRONI.—*Jour. de Phys.*

THIS contrivance consists of a moveable column, with a vessel of water proper for the purpose fixed upon it. This being placed under one end of any scale-beam, provided it be accurate, renders it capable of affording specific weights without having recourse to hydrostatic balances. One of the scales is to be left, and an exact counterpoise substituted for the other, having a hook attached to its upper and lower parts; the vessel being then filled with distilled water, and so placed that the body of which the specific gravity is to be ascertained, may always remain at the same height: this is easily effected by means of a rack and pinion attached to the column, by which it is moved upwards or downwards to the height required, and the substance to be examined becomes immersed under the surface of the water at the proper height. Thus any common scales are easily and simply converted into a hydrostatic balance.

The author then enters into some disquisitions relative to the specific gravities of several substances, the difficulty of ascertaining them with accuracy, and the opinion of several authors respecting the subject; but as these are foreign to the title of the paper, we shall not follow him through them.

Observations.—This contrivance has simplicity to recommend it, and will doubtless answer the purpose very well in many cases; but, though we thus readily subscribe to its utility, we do not think that it possesses all that convenience and accuracy which the inventor seems to attribute to it. The ingenious Mr. Prony invented a support for balances of all dimensions, whether common or hydrostatical, which is well calculated to render the operations to be performed, more expeditious and convenient, without diminishing their accuracy. This support, Mr. P. observes, besides the advantage of being adapted to all kinds of balances, whether they be furnished with suspending handles, or the beam have nothing but its centres, possesses that of being constructed at a small expense, either in wood or metal. A description may be seen in the *Annales de Chimie*, xxvi. p. 50; or in *Gregory's Mech.* vol. ii. p. 94.

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The substances to be weighed hydrostatically, according to the usual method, are suspended by a very thin wire: but by using *two* vessels of water, one under each scale, according to Mr. Prony's method, and suspending to the two scales, wires of equal diameter; one of them for the support of the substance that is to be weighed, and the other to be partly immersed in the water, their diameters will evidently have no influence upon the accuracy of the operation. The apparatus being so adjusted that the two wires may be in equilibrio at first, this equilibrium will be maintained, whether the beam be elevated or depressed, as it still retains its horizontal position: for thus the lengths of the wires, either above or below the surface of the water, being equal, the difference between the specific weight of the water and that of the metal will produce equal effects upon each end of the beam. It is obvious, that, if the wire by which the substance is suspended only be employed, the equilibrium established for one elevation of the beam will not be retained in other elevations.

It is also worthy of remark, that this method not only compensates for the excess of the specific weight of the wires above that of the water, but likewise for that arising from the adhesion of the fluid to the wires, as well as for the water which they carry along with them.

Table of Rain that fell at various Places in the Year 1808, *by the Rev.* J. Blanchard, *of Nottingham; with a meteorological Table for the same Year, by Dr.* Clarke, *of that Town.—Phil. Journ. No.* 103.

From this table of Mr. Blanchard's it appears, that, of the thirteen places enumerated where the observations were continued for the whole of the period, the greatest quantity of rain fell at Kendal, in Westmoreland, and was 43.34 inches, and the least at London, which is stated at 22.98 inches. At Chichester, Bristol, Chatsworth, Manchester, Lancaster, Dalton, and Kendal, the greatest quantity of rain fell in October, and was respectively 6.41, 5.26, 3.98, 5.32, 7.08, 6.53, and 7.25 inches. At London the greatest quantity fell in September, and was 4.18 inches. At West Bridgford, near Nottingham, in January, 2.85 inches. At Horncastle, in December, 4.10 inches. Ferriby and Kingston-upon-Hull, December, 5 01 inches. Heath, near Wakefield, March, 3.49 inches. Edinburgh, August, 7.51: the greatest quantity that fell at any of the above places in the space of one month. Also the annual quantity at Dalton was 39.99; at Chichester, 36.62; Lancaster, 32.48; Bristol, 32.06; Heath, 29.99; Edin-

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burgh, 29.34; Chatsworth, 28.81; Manchester, 27.09; Ferriby, 26.93; Horncastle, 24.32; West Bridgford, 23.22; and at Cheltenham, for the first six months, 12.60 inches.

From Dr. Clarke's meteorological table, it appears that the greatest height of the thermometer at Nottingham was on the 13th of July, and was 89°; least height, January 22d, 17°: greatest variation in 24 hours, January 22d and 23d, 19°; and the mean height 49.88 degrees. The highest observation of the barometer took place February 29th, and was 30.74 inches; the lowest November 18th, 28.72; greatest variation in 24 hours, October 19th and 14th, .91; mean height, 29.84. Greatest quantity of rain in April, 3.85 inches; least quantity of February, .54: total, 23.56. The number of fair days was 237, and of rainy days 128.

Observations.—Every cultivator of meteorology must wish that tables of this kind were more numerous, and that circumstances of this nature were carefully noted and recorded in a greater variety of places; as that is the only sure foundation upon which the superstructure of meteorology can be raised. One circumstance, however, is omitted in Mr. Blanchard's Table of Rain, which would have added greatly to its value, viz. the elevation of the several places, or rather of the different rain-gauges, above the level of the sea; that of Nottingham only being noticed, which is stated at 140 feet. The greatest height of the thermometer at Nottingham for July 1808 was 3° less than at London for the same month; and the former place is a little more than a hundred miles north of the latter, being in lat. 52° 59′ 35″ N.

Observations on the Progress of Bodies floating in a Stream; with an Account of some Experiments made in the River Thames, with a View to discover a Method for ascertaining the Direction of Currents. By James Burney, *Esq.—Phil. Journ. No.* 106.

Mr. Burney having frequently observed that laden barges, &c. on the river Thames, unimpelled by wind, oars, or towing, moved faster than the stream or the tide on its surface, was induced to inquire respecting both the fact and the cause. The several bargemen, whom he questioned respecting them, all agreed in the fact as being familiar to them. They added also, that a laden barge made a greater progress than a light one, and assigned as a cause that the laden barge had more hold of the tide: whence it may be inferred, that they supposed that the tide ran slower at the surface than at some depth beneath it. But Mr. B., from an experiment which he made with a stick and a riband attached to it, concludes that not to be the case.

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This writer considers the surface of a stream or current of water as not being horizontal, but an inclined plane, and the inclination of the surface as the cause of the current, and then has recourse to the effect of pressure in order to account for the circumstance. He observes, "an oblique pressure, not strong enough to submerge the body, effects it in two directions; one downwards in the manner of weight, to which the body yields to a certain and definite extent; the other horizontal, in which direction the body continually gives way to the pressure. Almost every person has experienced the readiness of a boat to glide from under him, on putting his first foot in her. These two effects of inclined pressure are separately in proportion to the whole pressure, one as the sine, the other as the cosine, of the angle of incidence is to radius.

If to a floating body on a horizontal surface a pressure is applied, in a direction making with the horizon an angle of 89° 59′, the proportion of the pressure which would act horizontally is to the whole pressure, as the sine of 1′ is to radius: and this proportion is $\frac{29}{100000}$ of the whole pressure. In like manner, if the surface incline 1′ from the true horizontal level, weight applied to a body floating on that surface will give an impulse towards the declining part of the surface equal to $\frac{29}{100000}$ of the weight applied. Consequently, a barge having in her 100 tons weight, floating with the stream where the declivity of the surface of 1′, will receive an impulse towards the declining part of the surface nearly equal to 65 lb. which is little short of what is estimated to be the average pull of a horse."

Mr. B. thinks it may be inferred from this, that if two pieces of wood, of equal size, but of different weight, were put into the water near each other, their separation would shew the existence and direction of a current, if any: and further, that the quantity of separation in a given time (effected by the heavier body leaving the lighter) might furnish a measure for the strength of the current. He then relates several experiments, undertaken with a view to ascertain the point, and made with globes of wood and sticks loaded at one end; but nothing conclusive was drawn from them, except that the heavier end shewed the greatest tendency to proceed down the stream. He and Mr. Rickman, his associate in these experiments, also endeavoured to determine the inclination of the river's surface, which they make 15″.

Observations.—From the time that we were called upon by our public duty to notice this interesting subject, we freely confess that we looked forward to the appearance of Mr. Burney's paper with high expectations of meeting with a complete solution of the problem; and it is hardly necessary to inform our readers, that the result has been a total disappointment. The experiments which Mr. B. made on the subject appear to have been singularly insulated, and much too loosely conducted to serve as the source of

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general and practical inferences. In fact, his communication bears no marks of a mind accustomed to assimilate, compare, and digest its ideas into a clear and perspicuous train of reasoning. We must, therefore, recommend a further prosecution of the subject to some person whom opportunity, ability, and habit, have qualified for the undertaking; and we doubt not but that the result of an extensive and well-conducted series of experiments would be a general theory of floating bodies.

Investigation of a Problem in the Doctrine of Permutations. By Mr. Peter Barlow.—Phil. Journ. No. 103.

Mr. Barlow, after stating that the problem had been considered only partially by all the authors with which he was acquainted who had treated on the subject, and observing that the rule which they have given for its solution is so tedious as to be of no real service in practice, proposes the following problem, and gives the subsequent rule for solving it:

Problem.—To determine the number of combinations that can be formed out of a given number of things, in which there are m things of one sort, n things of another sort, p things of another sort, &c. by taking 1 at a time, 2 at a time, &c. to any given number of things at a time.

Rule.—Place in a horizontal row $m + 1$ units, annexing ciphers on the right hand, till the whole number of units and ciphers exceeds the greatest number of things to be taken at a time by unity.

Under each of these terms write the sum of the $n + 1$ left-hand terms, including that as one of them, under which the number is placed; and under each of these write the sum of the $p + 1$ left-hand terms of the last line. Under each of these last the sum of the $q + 1$ left terms, and so on through all the numbers of different things, and the last line will be the answer: that is, the second term shews the number of combinations taking 1 at a time, the third term the number of combinations taking 2 at a time, &c.

They are succeeded by an illustrative example and an investigation of the rule, which we shall leave for the amusement of such of our readers as have leisure, analytical skill, and curiosity sufficient to induce them to undertake it.

Observations.—The simplification of abstruse branches probably ranks next to actual discoveries in the scale of general utility; and though it is not every reader whom education and ability have qualified to appreciate them, we are always glad when it falls within the province of our labours to record either the one or the other. Mr. Barlow seems well fitted to traverse the intri-

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cate labyrinths of analysis, to smooth down some of its rugged parts, and render the path more easy and pleasant for his successors. This appears to have been his object in the present instance, and his labours have been successful. His rule is simpler, and the process it requires shorter, than any we recollect to have previously seen on the same subject, and is therefore entitled to our approbation.

Observation of a lunar Rainbow. By L. Cordier.—*Journ. de Phys. Vol. LXV.*

Mr. Cordier states that on the 13th of August 1807 he, with several other persons, stood on an eminence where they had a clear view of the horizon, and that near them to the northward was a storm, and a copious rain, while the moon, nearly at full, rose in a clear sky towards the south. A luminous and well-defined bow appeared on the storm, in which however the seven primary colours were difficult to distinguish; but they seemed as if lost in a tint of pale yellow. It is also remarkable that the whole circle included by the bow was luminous, and tinged with a similar yellow hue, though not so intense as the bow itself.

Observations.—These phenomena, occasioned by the refraction of the lunar rays in the drops of rain, are not so rare as is generally supposed. Aristotle mentions his having been the first person who observed it; and adds, that it is never seen but near the time of full moon, as her light at other times is too faint to affect the sight after two refractions and one reflection. Mr. Alfeld collected accounts of about thirty of these phenomena that were seen prior to 1570; and many others have been observed at different times and places since that period. The lunar iris has all the colours of the solar; and in many cases very distinct and pleasant, but considerably fainter, both from the different intensity of the rays, and the different disposition of the medium. The circumstance of the two bows containing the same distinct colours is a proof that the opinion relative to their being of different natures was without any real foundation.

Thoughts on atmospheric Density and Pressure. By T. C. Speer, *Esq.—Phil. Mag. No.* 134.

Mr. Speer, at the commencement of this article, remarks, that notwithstanding the wide field of inquiry which the mechanical history of the atmosphere presents to us, and notwithstanding the interest which such inquiry must naturally excite in the philosophic mind, yet we must confess, our labours in it have been but

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partial, and our knowledge of it is but limited; the attention of men of science has (comparatively speaking) been mostly confined to its chemical history, probably from its more immediate connexion with, and relation to practical and useful results. Unaided by the lights of natural philosophy or the force of experiment, it scarcely comes within the limits of human conception, that that invisible, inodorous, aëriform mass of fluid surrounding our globe, should at all be subject to the laws, or possess the properties of matter; but particularly that it should possess either *density, weight*, or *pressure*."

As Mr. Speer conceives these three properties of the atmosphere to be frequently misunderstood, and confounded together, particularly the two former, he has made them the subject of the present essay. By the word *density*, Mr. S. informs us, that he understands *impermeability*, or that power in a body by which it is enabled to resist or obstruct (more or less) the passage of other bodies through it, and which may be estimated by the greater or less difficulty with which such resistance or obstruction is overcome. This *impermeability* is attributed to the closeness with which the component parts of a body are connected, in conjunction with their weight; so that he defines density to be that power *resulting from the union of the attraction of cohesion and gravitation.* Two well-known experiments are then stated to prove this property, viz. that of dropping two bodies of different weights from the same height, and that of a guinea and a feather let fall in the exhausted receiver of an air-pump.

The *permeability* of the atmosphere, or its property of yielding to the slightest impression, and affording little or no resistance to the insinuation of denser bodies into the interstices between its particles, arises either from the very slight aggregation of these particles, and their consequent small number and great distance from each other, or from their extreme minuteness, want of gravity, and spherical form, which permits them to slide amongst each other when any force is exerted on them. This latter hypothesis Mr. S. thinks the more probable; and that this tenuity or permeability may be estimated by the velocity with which heat, light, and sound travel through the air.

"*Atmospheric density* is generally confounded with *atmospheric pressure*, though distinct from each other; the one is a property it possesses in common with all other bodies, the other is peculiar to itself alone: in the one there appears an inherent and self-existent direction of the particles, and this direction seems positive and determinate; in the other there appears no direction but what is given by external and accidental causes, and therefore quite vague and indeterminate; and only what is possessed by all matter." As a proof of this pressure, the experiment of a tumbler, exhausted of air, and inverted in a basin of water, is cited. This pressure generally decreases in a direct ratio with

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its distance upwards from the level of the sea. If this ratio be supposed to be direct, and heights be taken in the atmosphere in arithmetical progression, its rarity (from want of pressure) will be in geometrical progression. Thus, as at seven miles above the earth's surface, the air is four times rarer, at 14 miles it is 16 times rarer, at 21 miles it is 64 times rarer, &c.

If it were not for the pressure of the atmosphere, there would be no other distinction between a fluid and a gas, than that which arises from the action of that aggregate attraction on one hand, and that quantity of caloric on the other, with which they are naturally endued: but this pressure, by uniting with the force of cohesion, weakens the power of the caloric, and thereby tends to keep the body in a liquid state. Hence the degree of heat necessary to convert a fluid into a gas, or to produce evaporation, must depend upon the pressure of the atmosphere: the greater the pressure, the greater degree of heat is required to produce the effect, and the contrary. When the united force of pressure and cohesion, therefore, is greater than the force of caloric which the fluid contains, liquidity remains; if less, evaporation ensues.

Mr. S. observes, that in the lower regions of the atmosphere the mercury in the tube of the barometer generally falls an inch for 850 or 900 feet in altitude at a medium: if this ratio hold good throughout the whole extent of the atmosphere, the mercury would remain in its bed, or not ascend the tube at all at the height of 27,000 feet. Mr. S. also gives the usual calculation for the whole pressure of the atmosphere, at 15lbs. per square inch of the earth's surface; as well as that part of it which is sustained by every common-sized man; and then states some reasons which render this pressure, according to the present constitution of nature, indispensably necessary to our existence.

Observations.—The uses of the atmosphere are so many and so great, that it appears absolutely necessary not only to the comfort and convenience of the human race, but even to the very existence of all animal and vegetable life, and to the very constitution of all kinds of matter; for by insinuating itself into all the vacuities of bodies, it acts as the great spring of most of the mutations which take place in these lower regions: as those of generation, corruption, dissolution, &c.; and without which, none of these operations could be performed. By its mechanical properties also many important purposes are answered. We employ it to give motion to mills, ships, &c. and for a variety of other purposes: and it is one of the great discoveries of modern philosophy, that the several motions attributed by the ancients to a *fuga vacui*, are owing to the pressure of the atmosphere. Dense air is the most proper for respiration, as relates to the mere quality of density only: but then the air in the higher regions of the atmosphere, though rarer than that below, is more free from phlogistic vapours; and hence

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it is that people can live very well upon mountains where the air is much rarer, than at the surface of the earth: it, however, appears that there is an intermediate height where the air is most salubrious and fit for the support of animal life; and this elevation has been stated, by M. Saussure, to be about 500 or 600 yards above the level of the sea. The Abbé Fontana, after treating on this subject, concludes by observing, that "nature is not so partial as we commonly believe: she has not only given us an air almost equally good every where at every time, but has allowed us a certain latitude, or power of living and being in health in qualities of air which differ to a certain degree. By this I do not mean to deny the existence of certain kinds of noxious air in some particular places; but only say, that in general the air is good every where, and that the small difference is not to be feared so much as some people would make us believe."

Surprising as it may appear, that the human frame is capable of sustaining so great an atmospheric pressure as that of near 14½ tons without seeming at all sensible of the load, it is still more surprising that the variations in this pressure should not be more felt by us than they generally are. It may be easily shewn, from the different heights of the mercury in the barometer (which ranges from 28 to 31 inches), that the difference in the weight of air sustained by our bodies in different states of the atmosphere, is frequently near a ton and a half: and when it is considered that the variation of the additional pressure of many pounds is often very sudden, it is not only a matter of astonishment that our bodies are capable of sustaining the change with so little inconvenience as we at present experience; but even that they do not suffer material injury from every such event. The fact is, however, that they always contain some elastic fluid, the spring of which is just sufficient to counterbalance the weight of the atmosphere.

What Mr. Speer has advanced as to the height of the atmosphere is probably not near the truth, as its altitude is considered by most philosophers as being indefinite, or, as others express it, as terminating in pure ether. One of its principal effects, however, being the refraction of light, the particles of which are the smallest of any which has yet come within human comprehension, it is not unreasonable to fix the limits of the atmosphere at the altitude where this effect is first produced. This altitude was calculated by the two celebrated philosophers Kepler and De la Hire, from the duration of twilight and the magnitude of the terrestrial shadow in lunar eclipses, and found to be between forty and fifty miles. At this altitude the air is supposed to be above 10,000 times rarer than at the surface of the earth. Twilight is known to begin and end when the centre of the sun is about 18° below the horizon, or only 17° 27′ when 33′ is subtracted for refraction. A ray transmitted by the sun in that situation, and enter-

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ing the atmosphere, is refracted, and caused to describe a curve line in passing through it to the eye. Different opinions have been entertained respecting the nature of this curve; but Heman shewed that it was infinitely extended, and had an asymtote; and Brook Taylor regarded it as one of the most intricate that could easily be proposed. The rays of the sun, however, may be reflected as well as refracted; and if they be supposed to arrive at the spectator, after suffering one reflection, the height at which this reflection takes place has been found to be about 39.64 miles, while the altitude at which they are refracted has been stated at about 77.25 miles: but the height determined by Kepler and De la Hire is as probable as any.

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1. *On the natural Causes which operate in the Formation of Valleys. By* JOHN CARR, *Esq.—Phil. Mag. No.* 134.

2. *An Inquiry into the terrestrial Phenomena produced by the Action of the Ocean, Nos. I. and II. By the same Author.—Phil. Mag. Nos.* 135 *and* 136.

1. MR. Carr considers "every river which disembogues itself into the ocean as the great drainage-trunk of a considerable extent of country, receiving through every part of its course lateral streams, which again receive others, and these others still, insomuch that the river itself is frequently the receptacle of hundreds of other streams of various magnitude and extent; and not only the river, but every brook, however remotely connected, has its peculiar drainage of valleys, which afford it the most easy and direct communication with the stream into which it falls; and all these ranges of vallies are as subservient to, and as intimately connected with the extensive and general system of drainage of the country, as the streams themselves; and the uniform direction, general connexion, and admirable subserviency of the whole, are so palpable, that we are irresistibly led to one of two conclusions —either that the several ranges of valleys have been purposely and specially formed for the streams which now flow through them; or that the streams themselves have scooped out their own peculiar valleys. The former opinion is too absurd to merit a moment's attention; and the latter has so many direct and positive proofs in its favour, as to yield the most satisfactory conviction to any impartial and competent mind, that will take an actual survey of the spring heads and courses of even the most trivial brooks."

The horizontal parallelism of the brows on each side of the valleys, and of the strata on their opposite sides, as well as the identity of these strata, Mr. C. considers as demonstrative proofs that they were once continued across the valleys, and that the formation of the valleys has been effected by these strata having

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been cut through, and the materials of which the intermediate parts were formed, carried away. The source of every stream is situated above the general level of the adjacent country through which the valleys have been formed; and it is conceived, that if each of these were filled up, there would still be a sufficient fall for the stream in its present direction. Its first course is supposed to have consisted of a chain of streams, water-falls, and lakes, according as the district through which it flowed, presented gentle declivities, abrupt descents, or hollows, until it reached the channel of some other stream, or even the grand receptacle of the whole, the sea itself. Mr. C. then enters into some detail respecting what would take place at the several water-falls; and concludes, "that the falls have worn out the valleys backwards into the lakes above, thereby giving vent to their waters, and leaving the course of the streams, as we now find them, a succession of valleys and alluvial flats." The circumstance of valleys commencing at the source of the stream, and gradually deepening as the distance from their commencement becomes greater, and that of the intersection of one valley with another at the confluence of two streams, Mr. C. regards as undeniable proofs of what he has asserted. In answer to such as object to this system, on account of the disproportion between the magnitude of the valley and that of the stream which is supposed to have formed it, he observes, "those occasional and powerful floods to which every brook is subject, the immense duration of the action, and the yielding nature of the earthy materials removed, are amply sufficient to suppress every doubt, and to reconcile every difficulty."

2. (No. 1.) Near the commencement of this article Mr. Carr remarks, that "a very moderate acquaintance with some of the ordinary phenomena of the earth will afford ample conviction, that over every portion of our present existing continents the waters of the ocean have rolled during a period to which we can assign no limit. Proofs of this extraordinary occurrence are abundantly disseminated over every part of every country. Wherever the earth has been penetrated, a succession of strata alternating and varying, distinctly and specifically, is met with under forms which unequivocally demonstrate that they have all originated from aqueous deposition; and the vast remains of marine productions, so extensively distributed among some, and in others forming whole and distinct strata, equally establish that they have all had a common origin. *They have all in succession been beds of the ocean.*" The regularly stratified beds which are perceivable in various precipitous parts of the country and of the shore, or which have been discovered by means of artificial perforations, every where present a uniformity of appearance, which Mr. C. thinks can only be explained by a slow and progressive precipitation from an aqueous fluid. As proofs of an extensive disruption he regards the denudations of numerous districts, the singular emi-

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nences of insulated materials which cannot be identified with any of the surrounding strata, and the highly elevated ranges of stratified mountains, which can only be the remaining fragments of extensive countries which have disappeared. "We are compelled to assign this disruptive transportation to the only natural agents whose gigantic forces and powers of mobility are in any degree commensurate to such mighty performances, the numerous and sweeping currents of the ocean."

Mr. C. then takes a survey of the bed of the ocean itself, in which extensive and accurate soundings in various tracts have fully ascertained that spacious horizontal plains are as rare below as above this grand receptacle of waters; and that inequalities of surface similar to those on land, from the gentle rise to the precipitous declivity, are no less common in the one place than in the other. With this corresponding similitude of surface, there has also been discovered an equally corresponding agreement in the conformation of its beds. It is likewise well known that the bed of the ocean is continually receiving fresh accessions of earthy materials, conveyed into it in union with the waters it receives: and this daily increase being repeated through a succession of numerous ages, must add vast quantities of various materials to extensive tracts of its bed. However, notwithstanding these important additions, Mr. C. thinks that "we can only look to the numerous and impetuous marine currents, sweeping with ponderous attrition, and irresistible force, over the abraded beds of the sea, for the cause of these stupendous phenomena, which arrest our attention, and elevate our astonishment in geological research." The precipitous faces and steep ascents which are observable in the hilly districts of every country, are conceived to possess a singular conformity to those which a vast marine current would necessarily produce on all the eminences in its course; and this is one of the circumstances which Mr. C. thinks utterly inexplicable by any other natural agency. He then enters into a fuller description of the effects produced by currents, and doubts not, that if their beds were laid open to our view, they would exhibit all the bolder features of geology with which we are acquainted. He also answers the objections, that the turbid waters charged with the particles of earthy matter, do not appear at the surface of the ocean, and that the transportation and formation of strata by the agency of marine currents, is only of local extent, like the currents themselves.

No. 2. "The two most inexplicable phenomena in geology, and which, beyond all others, have tempted the inquirer to wander out of the limits of natural operation into hypothetical regions of invention and surmise, are the various inclinations of the strata from horizontal up to vertical, and the elevation of the ocean up to those astonishing heights on our mountains, which bear the most unequivocal testimony of its former action." To

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account for these circumstances, and more particularly the latter, Dr. James Hutton supposed internal fires to exist in the lower regions of the earth, by the expansive force of which our continents, &c. were raised from the bed of the sea to their present heights. Mr. Kirwan, not satisfied with this explanation, supposes the ocean to have sunk into vast caverns contained in the bosom of the globe. And Mr. Farey has announced his intention of employing an extinct erratic satellite for the same purpose, which, by its near approach to our globe, reversed, for a time, the action of gravity, and thus gave rise to the present geological appearances. Mr. Carr, however, ridicules all these opinions, as "hypothetical reveries," and remarks on the subject that, "as far as we have yet made any successful discoveries into the operations of Nature, we find them all the result of permanent laws, which, acting with provident wisdom and rotative perpetuity, give a precision, a stability, and a duration, to every natural system, wherein no human calculation can trace out either a beginning or an end."

One of the most important queries, perhaps, relating to the science of geology, is, Whether the present interior conformation of our globe has arisen from one, or from several successive immersions of the same country? And this question Mr. C. seems to think absolutely necessary to be answered before any real progress can be made in establishing a rational theory of geology. He also states several circumstances which induce him to assent to a plurality of immersions. The principal of these are, that all stratified rock, not evidently the produce of marine shells in their native beds, has been formed from the consolidation of strata, previously derived from aqueous deposition; the numerous breaks and separations by which the strata are intersected; and the mineral veins in extensive fissures which traverse stratified rock. But, perhaps, the most incontestable proofs of distinct immersions are to be met with in those singular conformations of secondary horizontal strata over vertical strata of primary granite and schistus. Those extensive beds of sand and pebbles, which are found in every varying situation, from the tops of the highest mountains to the lowest valleys, and their respective situations, can only be referred to former shores of the ocean, and are so many proofs of separate submersions of the same country. Such, indeed, has been the wonderful transition of matter from one state and situation to another, that Mr. C. apprehends "it may be truly asserted, that there is not a cubic foot of material in any natural bed within the range of human inspection, that is not strictly derivative, having obtained its present form from its destructive dissolution in a former state; and that all our present continents have been constructed from the numberless remnants and fragments of other more ancient countries, evincing altogether periods of duration,

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systems of transition, and alternations of land and ocean, to which we can assign neither commencement nor termination."

Observations.—In our last Number, this subject came under consideration in two separate articles (see page 141 and 187); and in our remarks on the former of these papers, we stated our reasons for regarding the science of geology (whenever it can justly claim that appellation) as of considerable importance; expressed our conviction that very little, really valuable, was known with certainty on the subject; and gave it as our opinion that the *inductive* mode of reasoning was the only certain process that could conduct the geological philosopher to the attainment of his desired object—a rational theory. The collective body of facts is, however, yet too small to furnish sufficient matter for such an undertaking as this; and whenever time and experience shall have removed this grand obstacle, by increasing the accumulation, we think, with Mr. Carr, that there will be no need to light internal fires, to quench them by means of the ocean, or to employ a satellitious visitant, to account for the present appearance of our terrestrial habitation. Facts must constitute the foundation of the superstructure; and the present laws of nature, the architect's rule and plummet. Chance, mischievous irregularity, or accidental violence of action, have no place in the execution of these undeviating laws; they are only the blind applications of our partial views of things. The prolonged preservation of the ephemeral species, whose buzzing progeny commence and terminate their existence on the same day, is, as far as we can see and judge, as effectually provided for, and secured, as that of the solar system. And shall we look upon the stupendous and massive effects which the exterior and interior survey of our globe every where displays, as the production of, "and search for their causes amongst temporary and extinct agents? Shall so mighty an occurrence, in the scheme of terrestrial existence, as the stratified conformation and transition, arrangements of the vast materials of our earth, have no permanent established laws for their regulation and governance, while the fluttering insect of an hour is provided with its peculiar rules of formation and duration?" Such observations as these, and a great variety more of a similar kind that might be drawn from the same source, the regularity of Nature's operations, as well as the true spirit of philosophy, which has been so abundantly manifested during the last century, should, *we* think, operate powerfully in opposition to hypothetical reasoning, and induce the geological student to follow Nature and fact instead of fancy and imagination.

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1. *Geological Observations on the Excavations of Valleys, and local Denudations of the Strata of the Earth in particular Districts, &c. In reply to Mr. John Carr. By Mr.* John Farey. —*Phil. Mag. No.* 134.

2. *Geological Observations on the Excavations of Valleys, the supposed Existence of numerous Lakes, at former Periods, where Valleys now exist, which the Streams flowing through them are said to have broken down, &c. In reply to Mr. John Carr. By Mr.* John Farey.—*Phil. Mag. No.* 135.

1. Mr. Farey remarks on the necessity, in geological inquiries, of discriminating between the *extraneous* and the *local* alluvia of any district, and on further acquiring the knowledge, that *faults* or fissures, or whatever else they may be denominated, are the mere fracture and displacement of the files of strata, and do not in any instance affect the order, or nature, of the strata beyond their immediate vicinity. He then objects to Mr. Carr's position, that mountains are owing to the superior durability of the materials, of which they are composed, on the ground that the nature of these materials, in some instances, is too soft to have offered a sufficient degree of resistance to the operating cause, whatever it may have been. Mr. F. also entirely dissents from the opinion adopted by Mr. C. that the terrestrial strata could only be derived from the destructive transportation of other strata equally extensive, as being incompatible with both the regularity of these strata, and the organized remains imbedded in them; and regrets, "that Mr. Carr should have compared this exploded notion, with the *principle* of *gravitation*, as elucidated by Newton, T. Simpson, and La Place, to the entire accounting for all phenomena, to which it has been legitimately applied." Mr. F. then asks, from what source can Mr. C. infer that his present laws of Nature have been in action millions of years; and in opposition to what Mr. C. supposed to have been effected almost in an instant, he refers to his personal acquaintances for proofs of his having "all along supposed the probable period to be very long, during which a *satellite* revolved in a continually decreasing orbit, previous to its fall into, and its assimilation with, the compound mass of the terraqueous globe." He also supposes that the causes which effected the disruption and denudation of the strata, must have acted in a different manner from those which have exerted their influence since the last and great operations of creative power were performed, in the creation of the present race of vegetables and animals, and of *man*." When contemplating Mr. C.'s position, that the various materials which compose the dif-

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ferent strata, had been transported to their respective situations by means of water, a supposition forced itself on Mr. Farey's mind, that each successive stratum in the ascending series had been created since the animals and vegetables, whose remains it contains, had completed their growth; and that the deposition of such stratum had, perhaps, occasioned the successive extinction of these organized beings. Mr. F. likewise thinks, "that something analogous to the formation of hail in a storm took place during the precipitation of *silex*."

2. In this communication Mr. Farey denies the existence of alluvial flats in all the valleys which he has examined, as supposed by Mr. C. to have been the situations of ancient lakes that were dried up when the valleys were excavated by the streams which flow through them. He also remarks, that only a very inconsiderable quantity of native or local alluvia is to be found on each side of the grand central ridge of this island; but, on the contrary, much foreign alluvial matter has been deposited. Mr. F. likewise thinks, that the supposition of the valleys having been formed for the streams which flow through them, is not so full of absurdity as Mr. Carr asserts; and affirms, that valleys exist where no spring-heads are to be found, and down which no stream ever did or can flow, on account of the open nature of the hard rocks, amongst which these valleys have been excavated; and he cites the numerous deep dry vales extending from the Dove and Wye rivers, in Derbyshire, as well-known examples. Neither can Mr. F. grant the position that valleys always follow the lowest ground of the adjacent country, and he mentions some instances of the contrary.

That the angles of intersection of valleys and streams are acute above and obtuse below, and that two streams invariably meet on precisely the same level, are positions to which there are numerous exceptions.

Observations.—We are fully disposed to give Mr. Farey all the merit that is justly due to his indefatigable personal researches, to thank him for the many practical facts and observations he has already stated, and to hope that he will still add many more to the general stock before his labours attain their termination. In what relates to practical matters and personal experience, his communications are generally correct and valuable; and as far as they have been confined to a simple statement of facts, have usually been perused by us with approbation and confidence: but with regard to his assigned cause of the present geological appearances on the surface of this terrestrial globe, we have never yet been able to mark its place in the scale of creation, or to view it in any other light than that of a phantom of his own imagination.

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It is a well-known principle in nature, that the attraction of bodies, at a finite distance from each other, is reciprocal, proportional to their masses, and inversely as the squares of their distances. Mr. Farey supposes "the probable period to be very long, during which a *satellite* revolved in a continually decreasing orbit, and affected the stupendous operations on the strata, previous to its fall into and assimilation with the compound mass of the terraqueous globe." If the period was very long and the orbit continually decreasing, the distance between our globe and its approaching celestial visitant, when the action of gravity was first reversed at the surface of the earth, must have been considerable; and the ratio between the terrestrial radius and this distance, far from being small. But as attraction is directly as the mass and inversely as the square of the distance, the mass of the *satellite* must evidently have been immensely great when compared with that of the earth: hence (according to Mr. Farey) we have a large body revolving about a smaller—a circumstance which cannot take place according to the present laws of nature. Mr. Farey's language above quoted also implies an approach of the satellite towards the earth; and though their approximation to each other would be mutual, the less body or quantity of matter would pass through the greater space in the same time; and therefore the earth may be said, with more propriety, to have approached the satellite. Besides, he who can conceive two immense solid bodies nearly of a spherical form (and we have no reason to suppose that the satellite was of any other form), falling *into*, and becoming *assimilated* with each other, so as to constitute a compound mass of nearly the same figure, must possess a very extraordinary conception, and probably a sufficient degree of credulity to believe, that each of two contrary propositions may be true. Absurd as these conclusions may appear to which Mr. Farey's supposition naturally conducts us, equally so are the results of philosophic speculations in general, whenever the reins of judgment are abandoned to the capricious power of imagination, or the dim veil of prejudice permitted to envelope the *man*.

Remarks on some of the Definitions and Axioms in Barrow's Euclid. By William Saint, *Esq.—Phil. Journ. No.* 105.

In these remarks, Mr. Saint obviates the objections that have been urged against Barrow's 6th and 7th Definitions as wanting the word integral; and shews them to be agreeable with the author's definition of *number* and *part*. Definition 8th and 9th are objected to, as a number may be found which answers them both. The 15th definition appears to want the words *less one*. Definition 23d, axioms 7th, 8th, and 9th, Mr. S. thinks objec-

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tionable, as they imply the truth of the 16th proposition of the 7th book, which they precede. This proposition he has also given with Dr. Barrow's demonstration; to which he has added one of his own, accompanied with observations on the former.

Observations.—The objections which Mr. S. has urged in this paper seem to have some foundation, and his remarks in general to be such as naturally flow from the subject. Relative to the proposition above alluded to, considered in an insulated state, we do not exactly agree with Mr. S. respecting the necessity and importance of any demonstration at all: it has always appeared to us to be one of those self-evident truths to which we assent intuitively; and we think that no person who has learned the multiplication-table will hesitate in admitting, that when two numbers are to be multiplied together, it signifies nothing which of them be made the multiplier. And though this proposition, as Mr. S. observes, is to be found in Legendre's "*Essai sur la Théorie des Nombres*," it was more for the sake of preserving uniformity of method, than on account of its individual importance, that he gave a new demonstration. Upon the whole, we do not perceive what could have induced Mr. Saint to favour the public with this communication, unless it were the desire of appearing before them in a geometrical dress, and informing them, that, even at a watering-place (dated Cromer), he sometimes read an old obsolete book, first printed in the year 1655.

Account of an Improvement in the Formation of Anchors, and of an improved Mode of fishing them. By Captain H. L. Ball, *of the Royal Navy.—Trans. Society of Arts, Vol. XXVI.*

Captain Ball's improvement on anchors consists in forming and fixing the shank of the anchor to the stock, which is composed of two pieces of timber firmly fixed together with bolts and tree-nails, and well seamed by hoops, instead of being formed of one solid piece in the usual manner. Instead of the square projection which, in the common method, is forged on the shank to prevent it from slipping in the stock, Capt. B. causes a projection to extend on each side of the shank far enough to receive two of the bolts which join the parts of the stock together, and by which the shank is fastened to the stock. One of the hoops is also driven up between these bolts on each side of the shank, to make the union more secure; and the length of each of these projections is to be two and a half times the diameter of the shank.

The improvement in fishing the anchor which Captain B. proposes is to be effected by employing a double hook, for what is called the fish-hook, instead of a single one; and applying them to the arms of the anchor close to the shank, in preference to the

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shank itself. Two small ropes are fastened to the back of the hooks, for the purpose of directing them to their proper hold on the anchor.

Observations.—Those who are conversant with nautical topics know, that the stocks of anchors most frequently give way, or fail at the centre, and that the shank is liable to become loose in hot climates: both these inconveniences Capt. B.'s method appears to have a tendency to remove. The formation of anchor-stocks, according to the present mode, requires a considerable quantity of the finest timber; hence a saving of expense would be effected by having them made of two pieces instead of one; but it would require an additional quantity of iron and labour. Capt. B. states, that an anchor made according to his plan, "may be cheaper in construction," but from which we are induced to withhold our assent, from a persuasion that the expense of the additional iron and labour would more than counterbalance the saving in wood. The greater quantity of iron, however, would increase the weight of the stock; a circumstance which Capt. B. thinks would cause it to hold longer in the ground than other anchors; particularly "in banks which shelve suddenly down from the shore." Mr. James Stuard obtained a patent for an improved anchor in 1796, the specification of which was published in the fifth volume of the first Series of the Repertory of Arts; and in which, instead of wood, he proposes a bar of wrought iron, loaded or covered at the ends with knobs of cast-iron: which of these stocks ought to be preferred, we shall leave for experience to determine.

An Improvement in Ship-building, by securing the Ends of the Beams of Ships without wooden Knees. By Mr. THOMAS ROBERTS.—*Trans. Society of Arts, Vol. XXVI.*

IT is the introduction of iron knees instead of those of wood for the purpose of fastening the ends of the beams to the ship's sides, that constitutes this improvement; and Mr. Roberts states, that the beams of ships are more effectually secured by this means than by the common mode, at the same time that the necessity of standards, top and bottom riders, is superseded, and nearly of wood knees altogether. These iron knees are so formed as to be capable of being bolted to the ship's sides, the beams, and the chocks upon which they rest. The dimensions of these knees for a 74-gun ship are stated as follow; the upper deck flat iron knees, four inches in breadth, and one inch and a half in thickness; lower deck and orlop iron knees, five inches broad, and one inch and an eighth thick. This communication is accompanied by a certificate from the Commissioners of the Navy, stating the great utility and

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importance of the invention, and recommending to the Commissioners of the Admiralty that a pecuniary reward be granted to Mr. R. for his ingenuity, which the King was pleased to order to the amount of 800*l.* There is also subjoined a list of 23 ships which had been built according to this mode in the beginning of May 1808, and of seven more that were then building.

Observations.—Few persons who are acquainted with the subject will deny that the invention of more effectually securing the beams of ships to their sides constitutes a very important improvement in the practical part of ship-building; particularly when the violent motion they suffer in strong gales of wind, and the great number of ships and brave seamen that have perished from ships first giving way in these parts, are taken into consideration. The great number of knees that are required for a large ship (in the Victory, of 100 guns, there are about 360 lodging knees alone), the very high price of timber of this kind, the difficulty of procuring it, and the delay which is frequently occasioned thereby, render any easy and practical means of either diminishing their number or superseding their use, a very desirable object. Though we are not at all inclined to dispute the utility of the method now proposed (under certain circumstances), with those by whom it has been sanctioned, and who have asserted it to be an "object of great importance in ship-building," we shall only remark, that the bolts by which iron knees are fastened are only prevented from drawing by the head and clench of the bolt, while in fastenings of wood the bolts are driven with a strong drift, which, added to the head and clench, affords an additional security. From the same certificate that states the utility of Mr. Roberts's invention, we also learn that it was adopted from *absolute necessity*; if such, however, be its utility, should not *that*, and not necessity, have been the cause of its adoption? The East India Company substituted iron knees for those of wood in their ships for some time prior to the year 1802. In the month of May in that year Mr. John Whitley Boswell obtained a patent for building ships and other vessels, one object of which was to supersede the use of knees: see Repertory of Arts, Second Series, vol. ii. page 81, and vol. iv. p. 430. In the September following a patent was granted to Mr. Joseph Brindley "for certain methods of more effectually securing ships' beams to their sides:" in these methods the lodging knees are altogether excluded; but for the means by which this is effected, we must refer to the specification of his patent, published in the third volume of the Repertory, Second Series, page 1.

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An Improvement on Jury-masts. By Captain WILLIAM BOLTON, *of the Royal Navy.—Trans. Society of Arts, Vol. XXVI.*

IN this method of fitting up jury-masts proposed by Captain Bolton, proper partners or pieces of timber are to be bolted to the quarter-deck for two spare masts to rest upon, one on each side of the broken mast. Mortises are made in the partners to receive the lower ends of these masts, which are made square, and they are secured to the broken mast by two caps fitted by mortises to a square form on its centre at the top. "A strengthening cap, moveable on these additional masts, connects them, and the upper parts of these masts are secured firmly by trustle-trees in the main-top. The foot of a square fore-topmast passes through a cap made from strong plank, mortised into the heads of the two temporary masts above mentioned, goes through the main-top, and rests in the moveable strengthening cap, which connects those two masts, and enables the fore-topmast to be raised to any height which the main-top will admit, and be then firmly secured by the upper cap, the main-top, and the strengthening cap below it. The fore-topmast being thus adjusted, the cross-trees and topgallant-mast are mounted upon it in the usual manner." Captain B. also observes, that the two caps are the only parts necessary to be made expressly for the purpose, as the other articles are usually ready on board; and that the object of the lower cap is to steady and prevent any wringing of the lower jury-mast, and to fid the topmast when it is reefed.

Observations.—When masts have been damaged or broken, either in action or by stress of weather, a ready method of supplying the deficiency in such a manner as to enable ships to prosecute their voyage with the usual quantity of sail, is certainly an object of considerable importance in a country like this, where every contrivance which renders its accomplishment more easy, merits the encouragement, and its inventor the thanks of all who are concerned in naval affairs. This is what Captain Bolton proposes to effect by the method he has specified in this communication; and the readiness with which his jury-masts may be erected is evident, as they require no other materials than the spare spars which are usually carried on board of both King's ships and others, and so few of the parts require to be made solely for the purpose. The circumstance of the lower mast being double will allow the quantity of sail to be increased, and is therefore a proportional improvement.

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A Method of preserving the Lives of shipwrecked Persons, and of forming a Communication with Ships stranded by means of a Rope thrown over the Vessel from a Mortar on Shore. By Captain GEORGE WILLIAM MANBY.—*Trans. Society of Arts, Vol. XXVI.*

THIS method of Captain Manby's is the reverse of Lieutenant Bell's, for the same purpose, which has been noticed at page 302 of our last volume; and is nearly similar to that tried at the Cape of Good Hope, as mentioned in the succeeding page of that volume of our work. When the ball has been fired over the vessel and the rope lodged in the rigging, the persons on board "are to return a rope by the one sent, to which the people on shore will bend a stout rope, and a tailed block rove with the rope that had been fired, to get communication; these they will haul on board. The stout rope must then be made fast to any part of the rigging they can depend upon:" the mast head just below the cap is to be preferred, on account of the convenience of getting from the tops. The other end of the rope being made fast on shore, and the tailed block fastened just below the upper end of it; one end of the small rope, which is hove through the block, is to be fastened to the fore part of a cot, and the other end to the after part of the same article. The cot is to be properly fitted for being drawn on the stout rope to and from the vessel by the persons on shore; by which means persons and goods may be conveyed to land while the vessel holds together. Captain M.'s communication is accompanied by letters from a great number of gentlemen who have witnessed his experiments, and who bear testimony to the efficacy of his method, provided the necessary apparatus and persons acquainted with its use be at the place where it is wanted.

Observations.—In our remarks on Lieutenant Bell's invention, we mentioned some experiments that were then carrying on at Woolwich by Captain Manby, with reference to the same benevolent and praiseworthy object, and stated our opinion respecting the two methods, viz. that of firing from the *ship* and that of firing from the *shore*; to which remarks we must refer our readers on the present occasion.

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Description of an improved Screw Wrench, to fit different sized Nuts or Heads of Screws. By Mr. WILLIAM BARLOW.—*Trans. Soc. Arts, Vol. XXVI.*

In this instrument the screw-head or nut to be turned is held between two jaws, one of which is forged in the same piece with the handle; the other is moveable between two chukes, and fastened to the fixed jaw by means of a strong screw, which is fixed to the same jaw, passes through the moveable one, and has a nut screwed upon it. Another screw is tapped through the moveable jaw, having its point resting in a cavity in the fixed jaw. When it is required to adapt the wrench to any particular screw-head or nut, the nut upon the first of the screws must be loosened, and the latter screw screwed either in or out of the moveable jaw, until the opening between the jaws be of the exact width required: the nut upon the first screw is then to be turned until it presses upon the jaw, and holds it perfectly tight.

Observations.—In all cases, and particularly in machinery, where it is desirable that the screws and nuts should be firmly fixed without having their angles injured by the operation, it is evidently of considerable advantage that the wrench should exactly fit the head or nut: this is what is attained by the present invention. Its firmness will depend upon the strength of the principal screw, and the exactness with which one of its parts is made to fit into the other.

Observations relative to Mr. Gilpin's Machine for raising Coals, described at Page 74 of the Twenty-fifth Volume of the Transactions of the Society of Arts, and noticed at Page 305 of our last Volume. By Mr. GILBERT GILPIN.—*Trans. Soc. of Arts, Vol. XXVI.*

FROM these observations it appears, that Mr. Gilpin sent two accounts of his machine to the Society, and desired that the second might supersede the first, from which it had been varied in order to obviate some objections of ropemakers with respect to the durability of ropes. The account of this machine given in the Society's Transactions, however, was printed in part from one of these, and partly from the other, by which means the statement of the relative expenses given at page 306 of our last volume makes that of chains about one fourteenth of that of ropes: but a few lines above, it is stated at only one twenty-fourth; and Mr. Gilpin now gives it as his decided opinion that

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it will not even be so much as this. The Society, however, in this communication, have merely pointed out the existing difference in these two statements, without applying the means with which Mr. Gilpin had furnished them for their adjustment:—an *admirable* mode of correcting errors!

Improved Instrument for cutting Silk Shag Edgings. By Mr. PETER TANSEY.—*Trans. Soc. Arts, Vol. XXVI.*

THIS instrument or cutter is made of steel, and resembles in some degree the blade of a gardener's knife, being broader at one end than at the other, and sharp towards that end only. It is woven within the threads, and the cutting is performed merely by drawing it out with the narrower end first, so that the threads which are to form the shag may be cut by the sharp edge of the knife.

Observations.—That this cutting implement is very simple is evident from the above description, and is fully confirmed by Mr. Baker, a working cutler, who states that within fourteen days he made four hundred of them. Certificates were also received by the Society from ten gentlemen acquainted with the business, who assert its superior utility over any thing which had been previously invented for the same purpose; and the Society rewarded Mr. Tansey with a premium of five guineas.

A Method of preventing Doors from dragging on Carpets. By Mr. JOHN TAD.—*Trans. Soc. Arts, Vol. XXVI.*

THE machinery by which this purpose is accomplished, is constructed of a slip of wood, one inch and a quarter wide, half an inch thick, and of a length equal to the width of the door. It is covered with green cloth on the inside, and is hung to the bottom of the door, which is cut to a proper height above the floor, by means of three small hinges. When the door opens it is drawn up by a concealed spring, and passed down when the door shuts by one of its ends fitting into a concave semicircular piece of hard wood fixed to the bottom of the door-case, and which holds it down close to the floor or carpet, so as to prevent the external air from entering when the door is close. Mr. Tad states that it can be affixed to any door, and without causing it to be scarcely distinguishable from any other; that it will supersede the necessity of screw rising hinges, and that it is less expensive than other inventions for the same purpose. The communication is accompanied by a certificate from Mr. French of Wapping.

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who had employed Mr. Tad to apply this invention to two of his room-doors, and had "found it to answer to his satisfaction."

Observations.—This contrivance is simple, and we have no reason to doubt of its being effectual when properly applied; we therefore consider it as being worthy of adoption in a variety of instances.

Demonstration of a Theorem in the Diophantine Analysis. By Mr. P. Barlow.—Mathematical Repository, No. 9, New Series.

Mr. Barlow commences this investigation by stating the following *theorem*: "Every integral number whatever, is either a square, or the sum of *two*, *three*, or *four* squares." He also observes, that this theorem constitutes a part of the celebrated theorem proposed by *Fermat*, but left by him without a demonstration: viz. "Every number is a triangular number, or the sum of *two* or *three* triangular numbers; a square, or the sum of *two*, *three*, or *four* squares; a pentagonal, or the sum of two, three, four, or five pentagonal numbers; and so on."

In order to obtain a more simple and general solution of the above theorem than had previously been made public, Mr. Barlow makes use of the following propositions, which, together with the corollaries which he obtains from his demonstrations, we shall transcribe for the information of our mathematical readers.

Prop. I. "If A be any prime number, and all the consecutive squares 1^2, 2^2, 3^2, 4^2, &c. ..... $\left(\frac{A-1}{2}\right)^2$, be divided by A, they will each leave a different positive remainder."

After demonstrating this proposition in a very brief and simple manner, Mr. B. draws the following corollaries:

Cor. 1. "In the same manner it is proved, that the negative remainders are all different from each other."

Cor. 2. "Hence, also, we may see in what cases the positive and negative remainders are equal to each other."

Cor. 3. "When A is not a divisor of the sum of two squares (that is, when all the positive and negative remainders are different from each other), then some of each of these remainders will be *greater*, and some *less*, than $\frac{1}{2}A$."

Prop. II. "If A be any prime number, it is always possible to find four squares, w^2, x^2, y^2, z^2, such that their sum is divisible by A; that is, the equation $w^2 + x^2 + y^2 + z^2 = {}^1A.A$ is always possible."

Prop. III. "The product of a sum of four squares, by a sum of four squares, is likewise the sum of four squares."

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PROP. IV. "Every prime number is the sum of *two, three,* or *four* squares."

Mr. Barlow then applies these propositions to the demonstration of the proposed *theorem,* by which means this last is rendered extremely simple and easy. He also shews in a corollary, that whatever has been proved with regard to integral numbers, holds equally true with respect to fractions.

Observations.—Some time ago Mr. *John Gough* offered to the public what he deemed a demonstration of Fermat's general proposition, and of which we gave an account at page 297 of our vol. iv.; and our mathematical readers will easily recollect the controversy to which this gave rise between Mr. Gough and Mr. Barlow, and which we also noticed in the succeeding numbers of this work. Mr. *Legendre* gave a demonstration in the case of squares in his "*Théorie des Nombres,*" and which we believe was the only satisfactory one that has been published previous to this; which Mr. Barlow has not presented as original, but as a simplification and extension of Legendre's demonstration, by means of the first two propositions. And we conceive that each of our readers, who has made the analytical calculus his study, and is able to follow each of these writers through his demonstration, will agree with us, after perusing both, that Mr. B.'s labours have not been void of success.

REVIEW OF SPECIFICATIONS OF PATENTS,

PUBLISHED IN THE REPERTORY OF ARTS, DURING THE MONTHS OF JULY, AUGUST, AND SEPTEMBER, 1809.

Mr. JONATHAN DICKSON's *Patent for various Improvements in the Construction of Tuns, Coolers, Vats, and Backs, used by Brewers, Distillers, and others. Dated November* 1808.—*Repertory of Arts, No.* 86, *Second Series.*

ACCORDING to this specification, the various vessels used in breweries and distilleries, generally made of wood, Mr. Dickson constructs of iron or other metal, by fixing firmly together a number of plates, properly jointed; these joints he secures and makes water-tight by means "of some animal, vegetable, or mineral substance, prepared in the form of a paste, or in their natural state, as circumstances may require." In cases where the vessel is to be exposed to a considerable degree of heat, a cement consisting of pulverized iron, and a small portion of salts and sulphur, is to be used instead of the above-mentioned paste. This cement will corrode and unite to the iron, and will bear a degree of heat little less than the iron itself.

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In the bottoms of his coolers, Mr. D. fixes a number of pipes or tubes of any height greater than the depth of the vessel, for the purpose of admitting a free passage for the colder air of the atmosphere, which considerably hastens the cooling of the liquor. This contrivance also admits of the vessels being made to occupy less room, at the same time that they produce equal effects as in the usual way. When any of these metallic vessels are found to affect the fluids contained in them, this patentee lines them with either wood or tin, which gives them the properties of a wooden or tin vessel, according as they are lined with the one or the other.

Mr. D. also "declares, that all aqueducts, reservoirs, docks, canals, and all such places requiring inverted arches, and other securities from the innovation of the surrounding earth and water, may by the aforesaid methods, and of the aforesaid materials, be lined or otherwise secured, in place of wood, stone, and brick-work, used in common cases, so liable to give way." He likewise further states, that "strong warehouses, manufactories, fortified places, stately mansions, elegant palaces, tombs, monuments, churches and other places of divine worship, with their steeples or towers, to any extent, can of the aforesaid materials, and by the aforesaid principles, methods, and applications herein before described, delineated, ascertained, and made known, be permanently made, built, or erected, agreeably to any of the orders of architecture, in place of stones, bricks, and other terraqueous substances made use of in the common way."

Observations.—We doubt not but vessels of Mr. Dickson's construction will be found to answer the purpose very well, particularly when they are to be used in an exposed situation, as their durability will more than compensate for any increase which they may occasion in the original expense. Its tendency to decrease the consumption of a foreign article by the introduction of one of native origin, we also regard as an advantage.

Mr. George Finch's *Patent for manufacturing various Kinds of Metal Laces, so as to imitate Gold and Silver Laces; and also of manufacturing Gold and Silver on Laces. Dated February 1809.—Repertory of Arts, No. 86, Second Series.*

As this specification abounds in technical terms, and is not susceptible of being much abridged so as to preserve the exact sense of the original, we shall transcribe a small part of it in Mr. Finch's own words: "For manufacturing yellow copper lace; the wire to be drawn down to about the size of fourteen-penny silver, or finer, to be flatted, and spun in the engine upon fine thread, yarn, incle, or cotton, of a yellow, orange, or lemon

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colour, of various shades or tints, as the work may require, and to be woven upon fine thread, cotton, yarn, or incle, the size of spun silk, of the same colour, shades, or tints; the warp to be weighed as a spun silk warp, and to be woven in a loom. For manufacturing yellow copper wire lace; the wire to be drawn down to about the size of fine twenty-penny coppers, to be shot upon a fine warp of cotton, weighed as a spun silk warp, and to be woven in a loom." Similar directions are also given for manufacturing yellow copper French braid lace; yellow copper Prussian braids; yellow copper satin braid and other braids; yellow copper open lace; white plated copper lace; white plated copper wire lace; white plated copper French braid; white plated Prussian braids; white plated copper satin braid and other braids; white copper plated open lace; gold superb open lace; and silver superb open lace; but as we apprehend that any account of them would be uninteresting to most of our readers, they are omitted.

Mr. THOMAS WELL'S *Patent for an improved Method of making and constructing Barrel Cocks and Water Cocks. Dated June* 1809.—*Repertory of Arts, No.* 87, *Second Series.*

COCKS of this kind may be made of any sort of metal of which they are usually constructed; and the patentee states, that "their essential property and superior excellence depend on the construction of that part of the cock called the barrel, which contains the key or plug," and which is so formed that it cannot leak. This is accomplished by making grooves in the inside of the barrel about one eighth of an inch deep, and filling them up with leather, or any other similar substance that will absorb the wet. The number and direction of these grooves is said not to be of any consequence.

Observations.—If this mode of construction will effectually prevent leakage, as the patentee asserts, and when leather is used for filling the grooves we do not perceive any reason to the contrary, it no doubt merits the name of an improvement: and it admits of a much simpler construction than has lately been employed for attaining the same object.—See Mr. Massey's patent, page 207.

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Mr. JOHN WARREN's *Patent for an Apparatus to prevent Chimnies from smoking, and to extinguish Fires in Grates and Stoves without making any Dust or Smoke injurious to the Room or Furniture. Dated September* 1808.—*Repertory of Arts, No.* 87, *Second Series.*

THIS specification states two distinct objects, each of which requires a separate apparatus for its accomplishment. That which is to be placed on the top of the chimney is intended "to cause a continual current of smoke until its exit into the air, and to disperse it without any return into the chimney." It consists of two retort flues, and two or four dispersing flues, and is furnished with a smoke-divider, &c. Its diameter is to be nine inches, and its elevation one and a half times as much: but for the dimensions and elevation of its separate parts, we must refer to the figure in the number of the Repertory from whence this account is taken.

The other apparatus which the patentee has described, is for the purpose "of extinguishing fires in grates and stoves, &c. without making any dust or smoke injurious to the room or furniture." This is composed of a plate of metal fixed in the breast of the chimney, so as to slide down either in part, or wholly, before the fireplace: the former to prevent the chimney from smoking, and the latter to extinguish the fire. It is to be made to move in grooves from the mantle to the hearth. A weight nearly equal to the weight of the plate is to be attached to it by a line, or chain, passing over pullies, and acts like that of a window-sash. The whole apparatus is about two inches thick, and requires to be let that depth into the breast of the chimney, in order that the covering may line with the brickwork. If made to act without weight and pullies, it will require two springs, one at each bottom corner near the edge.

Observations.—The apparatus to be placed at the top of the chimney, is intended to cause the smoke to pass into the atmosphere without any lodgment by the way; and also to prevent its return again into the chimney, by excluding the weather from having any immediate access to it. It is in this last circumstance, we apprehend, that the chief utility of this contrivance will be found to consist, particularly in many situations; and this opinion is greatly strengthened by the fact that a considerable number of the chimnies which are subject to smoke, are only so when the wind blows from some points of the compass. With respect

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to the second part of this apparatus, or that for extinguishing fires by means of excluding the air, there is certainly nothing new in the idea that can merit the name of an invention.

Mr. Joseph Cuff's *Patent for Machinery for slaughtering Hogs, Bullocks, and other Cattle. Dated August* 1808.—*Repertory of Arts, No.* 87, *Second Series.*

In Mr. Cuff's method of slaughtering, a string is put round one or both the hind legs of the animal, which is then drawn up, by means of machinery, into a bay or recess constructed in the wall: so that the several animals or carcases are separated from each other. The machinery made use of may be of any well-known construction, which will give power or mechanical advantage to the man who is to be employed in raising the animal. The method of performing this operation is by means of a common tackle, and is too simple to need any description in this place: the patentee affirms that the business is carried on "without the possibility of accident, and with much fewer men than would have been required for the same business in the common way of slaughtering."

Observations.—We doubt not that this method of Mr. Cuff may facilitate the business when applied to small animals; but we do not apprehend that it can be so well used for those of the larger species. Tackle of the same kind has long been applied in slaughter-houses for raising oxen, &c. after they had been killed, and from this it did not require much stretch of invention to apply it to them while alive. The circumstance of having each animal drawn up in a separate recess, may, perhaps, have its advantages; but in our estimation, it possesses its inconveniences also.

Mr. William Proctor's *Patent for improved Methods of raising or supplying Tubes or Lamps with Oil, so as to remove the Shade of the Vessel containing the Oil. Dated March* 1809.—*Repertory of Arts, No.* 87, *Second Series.*

This lamp consists of a tube of metal, made so true, that a plug, or piston, of leather, cork, or any other elastic, tight-fitting substance, will contain the oil about it; and it is constructed in such a manner, that the tube will move either up or down over the plug, or piston. By this means the oil is caused to ascend to the upper part of the tube, or candle, like part of the machine. This contrivance is intended to render the appearance similar to

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that of a common mould or wax candle, and may be fitted to any kind of sticks by a screw, &c. Or it may be made with plain tubes, without the spiral screw.

Observations.—The form of this lamp, and the circumstance of the shade arising from the vessel which contains the oil in others being removed, we suppose will prove recommendations; for custom has rendered the figure and colour of a candle so familiar to us, that any thing which is designed to afford light, and assumes that shape, will have a kind of natural preference; and will, probably, operate more powerfully in reconciling us to the change, from the use of the one to that of the other, than any other shape, or form, would have done.

A lamp possessing the exterior shape of a candle, was invented by Mr. Alcock, of Kilkenny; for which he obtained a patent in 1806, and, in our opinion, is superior to Mr. Proctor's. It is described in our third volume, page 89.

Mr. Robert Ransome's *Patent for Improvements in the Wheel and Swing Plough. Dated May* 1808.—*Repertory of Arts, No.* 88, *Second Series.*

These improvements consist in fitting a variety of parts (of wrought or cast iron) to one frame, in order to adapt the plough to all kinds of land. By these means, the patentee says, "many advantages are gained, particularly in preventing the adhesion of clays or stiff soils, which occasion so much friction and inconvenience in the working of the common ploughs." As this specification is accompanied by 13 figures, representing the various parts, any description independent of the plate would much exceed our limits; we must, therefore, refer such of our readers as are interested in the subject, to the 88th number of the Repertory.

Observations.—These improvements, with respect to both their variety and formation, appear to have been carefully adapted to the different kinds of land for which the plough was required.

Mr. Thomas Noon's *Patent for Improvements on Guns, Pistols, and other similar Fire-arms, and which Improvements are applicable to Cannon and other large Guns. Dated May* 1809.—*Repertory of Arts, No.* 86, *Second Series.*

The parts of the lock which have been improved, are the pan, the hammer, and the plate. Those parts of the hammer and pan

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next the barrel, or breech, near the touch-hole, are chambered out, and made to receive a swell or bulge left on the barrel, which is also hollowed round the swell, or bulge: a hole is likewise pierced through the plate under the pan, at the bottom of the chamber. By these means, when the lock and barrel are joined together, any water, which may get between them, will run down the projection to the hollow in the barrel, or breech, and from thence escape through the hole under the pan without touching the powder it contains, provided the chamber and the projection be made to fit close to each other.

Observations.—These improvements appear well calculated to answer the end for which the patentee designed them, viz. that of keeping the powder dry in wet weather. The chief part to be attended to in the construction of the lock appears to be that of making the pan fit close.

Mr. Joseph Ilett's *Patent for producing fast Greens on Cottons, and various other Articles. Dated February* 1809.—*Repertory of Arts, No.* 86, *Second Series.*

This invention consists in combining a yellow mordant with indigo blue. Nine pounds of indigo is to be ground, with half a pound of orange orpiment, but which addition may be omitted, and 4 quarts of water; to this, 3lb. of gum Senegal dissolved in 4 quarts of water is to be added, and the whole ground together. This prepared indigo is then mixed with half as much by measure of the mordant, or more, if a lighter colour is required.

The mordant is prepared by dissolving grain tin in a mixture of 12 parts of muriatic acid mixed with 1 part of nitrous acid; the solution is then to be either boiled until 2-3rds are evaporated, or neutralized by adding as much caustic alkali as possible without precipitating the tin. This caustic alkali is prepared by boiling for an hour 1lb. of fresh slaked lime, and as much potash, pearl-ash, or soda in a gallon of water, and filtering the solution.

The goods are to be dipped in the usual way, but not drained, for they must be moved from one vat to another as quick as possible. They are then to be cleansed in a mixture of 1 part of oil of vitriol with 150 of water, washed as usual for raising in decoction of weld, or other yellow drugs, and branned or bleached until they become white.

Other solutions of tin, or sugar of lead, may be used; and the above proportions may be varied.

Observations.—The preparation of the caustic alkali seems to be very faulty. Mr. Descroizilles has exhibited the error usually made in this operation, as may be seen in his paper inserted in this work.

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Messrs. FREDERICK BARTHOLOMEW FOLSH *and* WILLIAM HOWARD'S *Patent for a Machine, Instrument, or Pen, calculated to promote Facility in Writing; and also a certain black writing Ink or Composition, the Durability whereof is not to be affected by Time, or Change of Climate. Dated March* 1809. —*Repertory of Arts, No.* 88, *Second Series.*

THE first of these inventions consists of a pen made of glass, enamel, or any sort of stone or metal through which a hole can be perforated. The hole at the point of the pen is very small, but larger above, and will hold a much greater quantity of ink than a common pen. The pen is polished at the point in the common way.

The ink, or composition, is made of Frankfort black and fresh butter, in equal weights; the mixture being ground very fine on a smooth stone. While the composition is warm, it is to be smeared regularly over one side of a sheet of thin wove paper; and after this has lain a day or two, the superfluous black is to be rubbed off with a dry cloth. The sheets are then to be put into a press, with a sheet of blotting-paper between each, and pressed lightly for about forty-eight hours. The paper thus prepared is generally fit for use in about a month afterwards.

The object to be attained by the union of these two inventions is that of producing several copies of the same piece of writing by one operation; and it is thus effected: take a smooth plate of any hard substance, at least as large as the paper, and place under it; then place a sheet of the prepared or inked paper between each sheet of writing paper, with its blacked side downwards. The several papers being kept from moving, and a sufficient quantity of common writing ink being put into the cavity or tube of the pen, it will flow from its point during the operation of writing; and the impressions on the lower sheets of paper will be formed by the inked papers placed above them. The best impressions are obtained when thin wove paper is used.

Observations.—The method here described of obtaining a plurality of copies of the same writing by one operation, we are inclined to regard favourably. The means employed certainly possess great simplicity, and are capable of being applied with facility and dispatch; much more so than when the same object is effected by means of complex machinery; a considerable difference in the original expense must also operate as a great recommendation of this method.

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Mr. Richard Scantlebury's *Patent for a Machine by which he counterbalances the Weight of any Column of Water, or other Fluids, required to be lifted by any Steam or Water Engine, or other Machinery, either worked by Animals or Men. Dated March* 1809.—*Repertory of Arts, No.* 87, *Second Series.*

This invention is founded on the principle of the inclined plane. The plane is to be made of suitable materials and dimensions, according to the purpose it is required to answer; and is to be hung on pivots at its centre, so as to admit of either end being depressed. On this plane a traveller or car is placed, about half the length of the plane, and is loaded at one or both ends with moveable weights. When these weights are moved from the centre of the plane towards either end, they will cause that end to be depressed to a certain degree; and *vice versâ*. These weights are moved by wheelwork, and the motion may be communicated by hand, or by connecting the work with cranks of water-wheels, mill-wheels, &c.; or by attaching it to the pistons or beams of steam-engines, or to any kind of rotary or reciprocative motion whatever. A fly-wheel may also be used to regulate the motion.

Observations.—Our readers will easily perceive that this specification of Mr. Scantlebury is insufficient for the illustration of its title, and that he has certainly omitted a very essential part—its application to the specified purpose. We can readily perceive that a plane suspended by its middle on pivots, may easily be depressed at either extremity, by the different positions of a weight placed upon it; but how this depression of the plane is to be applied so as to be effectual in counterbalancing a column of fluid, is not so easy to perceive.

Mr. Ralph Wedgwood's *Patent for an Apparatus for producing several original Writings or Drawings at one and the same Time, which he calls a Penna-polygraph, or Pen and stylographic manifold Writer. Dated February* 1808.—*Repertory of Arts, No.* 89, *Second Series.*

The principle of this invention, which is here called a penna-polygraph, consists in disposing of two or more sheets of paper in such a manner, that they may be written upon at the same time with a double-pointed pen. The pens which the patentee has described are to be held in the same way as a common pen. In the application of this principle, if the usual form of the paper to be

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preserved as a copy can be dispensed with, it is then cut into bars; this being laid upon a sheet of writing paper, and the penna-duplex, or double-pointed pen, being held with a proper inclination, one of its points will touch the bar of the upper paper, and the other the sheet beneath it; and these points being moved in parallel directions by the act of writing, necessarily produce two exact fac-similes. But when the usual form of the paper cannot be dispensed with, and it is required to write upon two skins of parchment or two sheets of paper, then this patentee makes use of a machine upon which the material to be written upon is fixed. These papers, &c. are moveable over separate frames, and so situated with regard to each other, that a point of the penna-duplex may rest upon each; and when one pair of lines is finished, by turning the handle of a roller one of these lines is caused to move upwards, and the other downwards, through equal spaces, so that one copy of the writing is always in sight. The lines may also be kept at any given distance from each other, by means of spring catches. For a particular description of the machinery by which this is effected, consult the Repertory, No. 88.

Observations.—The first of these methods which Mr. Wedgewood describes, can only be resorted to in some particular cases; the cutting of the paper into bars will occupy a considerable time, greatly deform the appearance of the paper, and render it much more liable to be torn than if it were whole. The machinery to be used when the form of the paper or parchment cannot be dispensed with, we think too complex, and its management too troublesome, for general adoption.

Mr. Samuel Brookes's *Patent for splitting raw Bull, Ox, and Cow Hides, so that each Side of the Hide so split may be manufactured for the same Purposes for which an entire Hide hath been before used; as follows: the Grain-side for Coach and Chaise Hides, and other Purposes; and the Flesh-side for Losh Hides, for white Leather, for Vellum, for tanning, and for other Purposes. Dated November* 1808.—*Repertory of Arts, No.* 88, *Second Series.*

As Mr. Brookes's specification is very short, and as a part of the first sentence contains the whole substance, we shall transcribe it: "Instead of splitting the tanned hides by the machinery of cylinder and knife, as has hitherto been done, or shaving the raw hide by a currier's knife; I do take the hides of bulls, oxen, and cows, in the raw state, immediately after the same shall have been unhaired, and I do split the said hides by machinery of like construction, but of larger dimensions than heretofore used; and

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by these means I do manufacture and produce two new materials; viz. the piece of hide having the grain-side thereon; and the other piece having the flesh-side thereon." He then concludes with a short sentence for the purpose of guarding against invasion of his patent-right.

Observations.—Mr. Brookes has here contrived to favour the public with something in a very short compass, which he calls a specification, or a description of an invention for which he has obtained a patent. In this short space, however, he informs us, that instead of splitting the hides "by the machinery of cylinder and knife," he does split them "by machinery of like construction, but of larger dimensions." What information is there to be obtained from this apparent contradiction, except that Mr. B. makes use of a knife that is a little longer than usual, and of a cylinder that is a little larger than that previously employed?

No. XXI. will be published February 1, 1810.

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RETROSPECT

OF

PHILOSOPHICAL, MECHANICAL, CHEMICAL AND AGRICULTURAL DISCOVERIES.

No. XXI.] *October, November, December.* [1809.

AGRICULTURE.

Experiments on Rye-grass. By Mr. JOHN WRIGHT, *of Pickworth, Rutland.—Agricultural Magazine, No.* 27.

In April 1806 Mr. Wright had a field of turnip land sowed with barley, which contained thirty-two acres. As he considered his farm tired with growing clover, he sowed this field with the following grass-seeds, as a comparative experiment: one part all trefoil; one part rye-grass and trefoil mixed in about equal portions; and one part all rye-grass. In 1807 these parts were all mown for hay, and all produced tolerably good crops; and the aftermath, as well as the growth of the following year, was depastured with sheep till spring 1809, when the lay was ploughed once, and oats harrowed in at the rate of five bushels to an acre. A very striking difference in the goodness of the crop was discernible in very early growth, and might be plainly distinguished as far as the land could be seen. A few days previous to the harvest the oats on that part of the field which had been sown with trefoil only, were estimated at five quarters to the acre; those on the part sowed with trefoil and rye-grass mixed, at four quarters to the acre; and those on the part sown with rye-grass alone, at three quarters to the acre.

Mr. Wright had also eighteen acres sown after rye-grass, which he believes will produce from four to six bushels an acre less than the rest of his crop.

Observations.—These remarks will induce agriculturists to pay more attention than has been usually done to the effects produced by sowing rye, or rye-grass; but the round numbers in which the produce is given, and that by *estimation* only, will not stamp

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this experiment with that credit to which it would have been well entitled if it had been conducted with greater accuracy. It is however worthy of notice, that sowing oats after rye-grass is almost a repetition of the same crop, for both of them draw their nourishment from a root which penetrates very nearly the same depth into the ground; whereas the root of trefoil lies near the surface, and seldom penetrates to the depth where the fibres of the root of the oat begin to expand.

Experiment on the Sun-flower. By Mr. John Wright, *of Pickworth, Rutland.—Agricultural Magazine, No. 27.*

From the notice attracted by the many papers in this Magazine respecting the cultivation of the sun-flower, this gentleman was induced to sow a considerable patch of it last year; the crop was very strong, and he could not estimate the produce at less than in the proportion of twenty quarters of seed to an acre; but it appeared to him a tedious crop to harvest, as it ripens at so many different periods; and he thinks that the heads ought to lie some time in the sun to dry after they are cut; for although his were gathered dry, and spread upon a boarded floor, yet they grew mouldy. The crop was not injured by sparrows, or tom-tits, or any other small birds; it was not even attacked by them. Having obtained some quantity of seed, he made a comparative experiment with it against buck-wheat in feeding poultry. He put up to feed two young barn-door fowls, and allowed one a quarter of a peck of buck-wheat, of the value of four-pence; and the other the same quantity of sun-flower seed, which he valued at two-pence; being 21*l.* 6*s.* 8*d.* for an acre of twenty quarters produce. The fowl which had had the buck-wheat weighed 3lbs. 2 oz. when put up, and consumed its food in fifteen days, having increased in weight seven ounces for four-pence, which is about nine-pence a pound. The other fowl weighed previously to fattening, 3 lbs. 4 oz. and consumed the sun-flower seed in thirteen days, gaining four ounces in weight, which is at the rate of eight-pence a pound.

It is inferred from this experiment (if it can be depended upon, says the writer), that *an equal weight* of sun-flower seed would have increased the fowl fed on it to *eight* ounces, while the buck-wheat produced only *seven*; its feeding properties however appeared much more tedious, and a doubt remained with him whether a fowl would have gone on well upon it for another thirteen days, without being what is called *pen-sick*.

Observations.—If we admit Mr. Wright's premises, his conclusion by no means follows from them. There is too much confusion in the account for us to pretend to unravel it. He states that

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he gave to each of the fowls a quarter of a peck of each kind of seed, which is an equal quantity by measure; and that the buck-wheat fowl gained seven ounces, and the sun-flower seed fowl only four ounces. Now for the latter fowl to gain eight ounces, it must eat double the quantity of food which produced four ounces; namely, half a peck of sun-flower seed; but how this comes to be of an equal weight with half the quantity of buck-wheat, is no where explained to our apprehension. We presume that Mr. Wright mistook, and wrote, *weight* for *value*; since if he does not mean that four pennyworth of sun-flower seed will produce eight ounces increase, while four pennyworth of buck-wheat produces only seven ounces of increase, we confess we are too stupid to understand him. And then it does not follow that the *weight* will be equal merely because the *value* is; and this is not probable when the *quantity* is double.

One inference is plain, though it seems to have escaped Mr. Wright;—it is, *that Mr. Saunders's sun-flower speculations are not worthy of further notice.*

Reply to Mr. Hunt. By JOHN WRIGHT, *of Pickworth, Rutland.—Agricultural Magazine, No.* 27.

Merino Mutton not morbid scrofulous Carrion:—in reply to the pert Apothecary of Loughborough. By Mr. JOHN FAREY.—*Agricultural Magazine, No.* 27.

On Merino Sheep, with miscellaneous Observations. By AGRICOLA NORTHUMBRIENSIS.—*Agricultural Magazine, No.* 28.

A patriotic Address to the noble Defenders of the Merino Cause, and other Gentlemen who have vainly hoped to rival Bakewell's Fame. By JOHN HUNT, *of Loughborough.—Agricultural Magazine, No.* 29.

Congratulation on the Recovery of a valuable Correspondent.—Resurrection of Mr. George Culley.—Merino Breeding in the North, &c. By CLERICUS ET COLONUS.—*Agric. Mag. No.* 29.

So far from the angry and ill-conducted controversy between Mr. Hunt and Mr. Wright having come to an end, it has branched out into new ramifications, and fresh writers have enlisted themselves on both sides. If any reader expects to find either instruction or information in these *five* papers relating to that controversy, the perusal will be only repaid with disappointment; but will afford a high treat to the lovers of scurrility and personal abuse.

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On Smut in Wheat. By T. Batchelor, *of Lidlington, Beds.—Agric. Mag. No. 27.*

This gentleman having had his attention particularly called to the subject of this paper by some observations in the Magazine, determined to commence a series of experiments, to ascertain, if possible, the nature and cause of the disease, as well as to discover whether or not it was infectious. The experiments were made on but a limited scale, as they occupied little more than two poles of a garden, sheltered from the north winds by a clipped hedge of more than four feet high. The ground had not been manured for the purpose, but was in rather better condition than arable lands generally are; and the kind of wheat which was subjected to the experiments was the common red lammas. The crop was pulled about the middle of August, and the roots and ears carefully numbered, as well as accurately sorted.

No. 1 was clean wheat rubbed with dry smut, and sown at the interval of an hour: of this 62 roots produced 538 ears of corn; 12 of these roots had all good ears; 15 of them all smutty ears; and the remaining 35 had some ears of both kinds:—the whole number of good ears was 246, and of smutty ears 292.

No. 2 was the same wheat soaked for an hour in water, in which smut had been thoroughly mixed: of this, 52 roots produced 443 ears, of which 264 were good, and 179 smutty; the different roots producing some all good ears, some all smutty, and some of both kinds, as before.

No. 3 was conducted the same as No. 1:—61 roots produced 384 ears; of which 156 were good, and 228 smutty.

No. 4 was conducted the same as No. 2:—63 roots produced 355 ears; of which 144 were good, and 211 smutty.

No. 5 was clean corn picked out from smutty ears:—8 roots produced 85 ears; 11 of these were blasted ears, 73 were good, and 1 only was smutty: some of the roots had all the ears good, and some mixed ears, but there was no root entirely smutty.

No. 6 and No. 7 were both clean wheat of different samples, and were sown without any preparation:—120 roots produced 933 ears, of which five only were smutty; three of these five were on the same root, which produced three good ears. As the proportion here was 183 good ears to one of smut, Mr. Batchelor concludes that it is nearly the same as happens in common crops, for he had no reason to believe that they were caused by any infection of the seed previous to sowing; and he conceives the case analogous to the gaol fever; canine madness, &c. among animals; which though known to be infectious, yet often originate in a manner for which no cause can be assigned.

He also observes, that though these plants were all sheltered

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from the north-east wind, yet there was mildew on every individual stalk. From this circumstance he denies the supposed connexion or affinity between *smut* and *mildew*, or that the north-east wind (as some have confidently asserted) is the cause of either.

The first four experiments are stated to have been made for the purpose of ascertaining whether the smutty powder possesses the power of propagation; and it is argued from the result that it has; because the difference of proportion between the smut in these, and in Nos. 6 and 7, was too great to have happened by chance, when the corn was all sown in the same day, on the same soil, and under similar circumstances.

No. 8 was smutted wheat dressed with hot lime, and sown after an interval of twenty hours: of this, 27 roots produced 243 ears, six of them only being smutty; and these were distributed on six different roots.

No. 9 was the same smutted wheat rubbed over with mercurial ointment, and sown 22 hours afterwards: of this, ten roots produced 107 ears, of which nine were smutty.

No. 10 was the same wheat washed in water, and soaked 23 hours: of this, 33 roots produced 275 ears, 98 of them being smutty.

No. 11 was smutted wheat dressed with lime, in the same manner as No. 8, and sown after an interval of 48 hours: of this, 27 roots produced 250 ears, and every one of them good.

No. 12 was smutted wheat soaked 54 hours in water: 20 roots produced 200 ears, but 68 of them were smutty.

No. 13 was dry smut: 19 roots produced 173 ears, of which 19 only were smutty; and these were distributed on six roots only.

No. 14 was clean wheat, bruised with a hammer (as bruising had been sometimes thought a cause of smut): only five seeds vegetated, which produced 81 ears, all of good corn.

No. 15 was smutted wheat, dressed with lime and urine, and sown an hour afterwards: 11 roots produced 103 ears, and all without smut.

No. 16 was the same wheat soaked in urine only, and sown at the same interval: 20 roots produced 131 ears, but 41 of them were smutty.

No. 17 and No. 18 were smutted wheat, dressed with lime and urine, and sown the first at an interval of three hours, the latter of six hours: No. 17 had no smut out of 194 ears; and No. 18 only three smutty ears out of 198.

No. 19 was the same smutted wheat soaked in urine only, for six hours, and there were 71 ears smutty out of 110.

No. 20 and No. 21 were wheat of a different sample, but equally smutty. These were each soaked for eight hours: No. 20, which was soaked in urine and lime, produced 55 ears of

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good wheat, and no smut; and No. 21 soaked in urine only produced 63 ears of smut to 41 good ears.

No. 22 was smutted wheat dressed with mercurial ointment, and sown at the end of five days; ten roots produced 57 good ears, and four smutty ears.

No. 23 was the same wheat soaked in water for the same time; 79 roots produced only 52 good ears, and 105 ears were smutty.

No. 24 was the same wheat dressed with lime, and suffered to remain also five days before sowing; the produce from 16 roots was 56 ears, of which 55 were good, and one blasted, but there was no smut.

From the result of the seventeen last experiments it is contended, that dressings are useful in obviating the smut in wheat, it being evident also that *all* are not equally useful, since that which was washed in water alone was proportionally more productive of smut than that which was dressed with urine, and that dressed with urine alone more productive of smut than that which was dressed with lime. The experiments, No. 16, 19, and 21, relate to urine alone, and the produce was in the aggregate more than two thirds smutty. It is inferred, therefore, that urine alone cannot be of any utility in preventing the infection of smut, and that these experiments form additional proof that the disease is infectious. The experiments, No. 8, 11, and 24, are on the power of lime as an antidote, which perfectly succeeded in No. 11 and 24, though it was not entirely infallible in No. 8. But, on the whole, the results were what might reasonably be expected by those who held lime to be the most effectual, though not in all cases an infallible, remedy. The experiments, No. 15, 17, 18, and 20, with a mixture of lime and urine, are noticed as very efficacious, but are not held to be sufficient to determine whether the virtues of lime are increased or diminished by a mixture of urine; and the same doubts are conceived to apply to salt, though no experiment was made with that substance. Smearing the wheat over with mercurial ointment, as in No. 9 and 22, was apparently useful in preventing the smut, but was at the same time prejudicial to vegetation, by excluding air and water from the corns: this unusual prescription was employed on account of its known efficacy in destroying vermin and animalculæ; and consequently it would have destroyed the smut, if caused by the depredations of the latter, as some have supposed. The general appearance of the corns was good, and they were found to be heavy.

According to the best observations of this writer, there were no previous tokens of smut, most certainly no appearance of blue mucor on the chaff, or sickly yellow on the ear; but the smut-balls might be distinguished as soon as the ear made its appearance. Mr. Batchelor has, both in this and former years, seen ears completely smutted that had never seen the light, and therefore these could not possibly have been burnt by the cold north-

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east winds. The distinguishing external character of a smutty ear is said to be its dark blue green colour, in which it is similar to the stalk below the ear; but the stalks and leaves of sound wheat and smutty wheat are stated to be precisely the same.

The opinion that smut is caused by cold north-east winds is combated with much successful argument; and these experiments, conducted on a spot completely sheltered from such winds, are referred to as decisive of that point; but it is admitted that *blasted* ears may be occasioned by lightning, or some other atmospheric cause: but as it is certain that blasted ears, like those of smut, generally proceed from the same root, it is contended, that to that root we ought principally to look for the cause of the disease; for these diseases may perhaps derive their origin in part from seminal infection, and in part from such substances as the root may meet with in the soil, or it may be a constitutional defect in the seed. Nor does it appear to this writer that the blight or withering of the leaves has much to do with the mildew, since the former happens early in the spring, and the latter late in the summer; and he apprehends it can never be seriously believed that the smut can be communicated to a growing crop, though it certainly may to the seed.

On a comparison of the powder of smut with wheat-flour, by using a microscope which magnified the diameter of the object about 120 times, the smutty powder appeared to consist entirely of globules perfectly similar in size and appearance, and partially transparent, as light was perceptible through the middle of them, and their apparent size, when magnified, was about one fourteenth part of an inch, and consequently their real diameter about the 1680th part; but the appearance of wheat-flour was considerably different, for this consisted of globules of various sizes, mostly about one half more in diameter than those of the smutty powder, and among these a quantity of smaller particles, of a size and shape scarcely definable. Mr. Batchelor has little doubt that the larger parts of wheat-flour are the starch, and that the smaller kind compose the vegetable gluten, which possesses much alliance to animal matter. The meal of a blasted ear of wheat consisted of particles much smaller than those of smut, and is therefore presumed to be a different substance: and though it might be thought, from the disagreeable smell of smut, that the vegetable gluten (the only part liable to putrefaction) was also the only part destroyed by that disease, while the farina, or material of starch, was only blackened by it; yet strong difficulties are asserted to oppose this opinion, the most material of which is the indissoluble nature of the particles of smut, which seemed to be little affected by the power of boiling water, spirit of salt, oil of vitriol, or aquafortis; but a solution of soda appeared to exert the greatest power on this mysterious substance, which, though it did not dissolve the globules, yet seemed to render many of them

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more white and pellucid, and separated from a substance, the particles of which were too small to be distinguished by the magnifying power employed.

If then the infectious nature of the smutty powder be acknowledged, and if the infecting substance consists of the relics of any putrid matter, it is contended that there is the strongest reason to believe that either *lime, potash, soda, spirit of salt, oil of vitriol, aquafortis, arsenic,* or *corrosive sublimate,* are capable of destroying or neutralizing this substance, so far as to prevent any infection from taking place: and, among these ingredients, *lime* is held to be the most eligible, both on account of its cheapness, and the little power it possesses of destroying vegetation; many instances, however, of its inefficacy may arise from the mildness of its operation, and it may be many hours before it can produce its full effect. This writer is of opinion, that the failure of lime to destroy smut may almost always be traced to an imperfect application; for it has been the unvarying complaint of many writers on husbandry, that "the seed is seldom steeped a sufficient length of time." And though he considers *lime* the most efficacious of all remedies for smut, yet he thinks there is no rational ground to expect a perfect cure of smutty seed, unless it be steeped in strong lime-water for at least twelve hours.

Observations.—We have been the more diffuse in our account of this communication from Mr. Batchelor, because of the importance of the subject to the agricultural world, and because it contains a plain detail of actual experiments, which will tend to ascertain a fact about which much difference of opinion has for a long time existed. These experiments prove, that smut may be communicated by infection, that its vegetative principle may be destroyed by steeps, and that steeps composed of *lime* are the most efficacious. The influence of cold winds from a particular quarter, in producing this disease, is proved to have no foundation, and the doubt whether *smut* and *mildew* were the mutual causes of each other is set at rest, since they are shown to be diseases completely different. The comparing of the powder of the smut-ball with wheat-flour, by microscopic observations, reflects much credit on Mr. Batchelor's talent for investigation; and the result of that comparison, when extended to the powder of blasted ears, fully establishes the opinion, hitherto doubtful, that the blast is also a disease distinct from the smut. It is, however, to be regretted that the experiments were not extended to wheat steeped in a strong brine made of salt, as this measure is resorted to by most of the farmers in the midland counties, who usually mix the corn so steeped for twelve hours with hot lime, and immediately carry it to the field to sow. Now, though lime may be an infallible destroyer of smut (which we believe it to be in most cases), yet here is not allowed sufficient time to produce the effect, as

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the lime cannot be intimately mixed with all the seed by merely turning over once with a shovel, which is the usual practice.—Those who agree with Mr. Batchelor in the efficacy of lime for this purpose will discover many valuable hints in this paper, and those who hold an opposite opinion will find it extremely difficult to discredit his experiments, or disprove his reasoning.

On Essex and Berkshire Pigs, farming Accounts, &c. By C. C. *of St. Albans, Herts.—Agricult. Mag. No.* 27.

The original breed of Essex pigs is described to be perhaps as ancient as any one in Britain; to be invariably erect in the ear, long-headed and bodied, roach-backed and thin-haired, bone not large, very large consumers, and very mischievous in breaking fences; the largest of them capable of being fattened up to between forty and fifty stone, of 8lb. to the stone; thin-skinned, quick feeders, and carrying a considerable quantity of flesh, in proportion to their bone. The improved Essex breed, which is sometimes called the agricultural pigs, is said to have nothing of the Berkshire in them, the variety having been solely formed by a repetition of foreign crosses.

In regard of the idea of one breed paying more for corn feeding than another, this writer scarcely thinks it correct, since none can ever be made good pork or bacon without some kind of corn or milk, nor would any pig, in his opinion, pay for being kept on corn for a month or six weeks, unless it were half fattened before. Potatoes are condemned as a food for pigs, even when mixed with corn, because it is conceived that the quality of the flesh will be deteriorated in proportion to the mixture of potatoes; since pork or bacon fattened entirely upon potatoes is most flabby, insipid, and unsubstantial, and not worth, for any real use, half the price per pound that good meat is; for the quantity of it, which will boil or roast away in dressing, is great indeed. Roots and greens are recommended as very good food for store pigs; but there this feeding should stop.

It is stated that prick or erect eared pigs are always the quickest feeders, chiefly on account of their smaller bone and size and more compact form. "Hence (says the writer) they are *best* adapted to pork."—"But (we continue to quote his own words) there is a breed of small lop-eared pigs far superior to any for pork, whether in respect of weight or quality of flesh; such of the highest character are those found in Oxfordshire and Buckinghamshire." . . . . . .

The observations on farming accounts are only complaints of the difficulty of keeping them.

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Observations.—When the writer under our notice had assured us that there was a breed of lop-eared pigs, *far superior for pork to the best*, we thought it most advisable to conclude our account of his literary production, lest we should be enveloped and intrenched in absurdities and contradictions too deeply to make our escape; and we determined to retreat in time.—If that pork, *which is far superior to the best*, was an infallible antidote to the *cacoethes scribendi*, we would recommend a good dinner of it to *G. G. of St. Albans, Herts.*

On feeding Cattle with Sugar. By a CONSTANT READER, *of Whitehaven.—Agricult. Mag. No. 27.*

On Sugar as Cattle Food. By PHILO-CONJUROR, *of Croydon, Surrey.—Agricult. Mag. No. 28.*

As the latter of these papers was written in reply to the former, they are both noticed under one article. The inquiries of the *Constant Reader*, as to the advantages of using sugar as a food for cattle, are answered with more ridicule than is consistent with any desire of giving information. It is, however, agreed, that neither sugar nor molasses can ever be resorted to for this purpose on account of their high price, except perhaps in the immediate vicinity of the metropolis, where there are no expenses of carriage.

The circumstance of young cattle having been reared by artificial food of any kind proves nothing, unless they become of superior worth, or are reared at less expense, than when the usual method is adopted.

On the Alderney or Norman Cows. By J. T. of Patrimoine Cottage, St. Lawrence, Jersey.—Agricult. Mag. No. 28.

THE writer states that he resides in the island of Jersey, where he cultivates a small farm for his amusement, rather than for profit; he keeps five or six Alderney cows, as well as some of the English breed, which enables him to point out the difference between them. The purchase of an Alderney is said to be much less than that of an English cow, the price of the former being from six to nine pounds; and it is confidently represented, that the nourishment, which would be sufficient for a large English cow, would amply supply two of the Norman breed, as they are not so delicate in their food, as well as requiring a smaller quantity; their milk is admitted to be less abundant in quantity than the English, but is thought very superior in quality, and often produces twelve or thirteen pounds of butter in a week from one cow. For light pasture and heath lands the Alderney are recommended as far superior to the English cows.

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Observations.—This may be a very accurate picture of the Alderney or Norman cow in the island of Jersey, but the statements by no means agree with the circumstances that take place on this side the channel. The milk of the Alderney cow *here* is abundant in quantity, but far inferior in quality, to that of the Sussex or Devonshire breeds, though perhaps it have some superiority over that of the Holderness cow. Alderney cows have, almost universally, a thin and lean appearance in England.

Experiment on drilled and broadcast Wheat. By T. B. *of Sussex.*—*Agricult. Mag. No.* 28.

The experiment, which forms the subject of this article, was made in a small enclosure, which was very poor, and overrun with all sorts of weeds, furze, and broom. These were stocked up and burnt, and such a border was left in grass as for the arable part to consist of exactly three acres. The first crop was winter tares, which was followed by Swedish turnips, and then by lucerne, which was but an indifferent crop, being almost choked by couch and other grasses and weeds. This crop was ploughed up, and the land completely cleaned, in September. One acre was drilled with wheat in rows at an interval of eighteen inches; one acre was also drilled in eight-inch rows; and one acre was sown broadcast. The eighteen-inch rows took a bushel of wheat within less than a pint, the eight-inch rows two bushels nearly, and the broadcast two bushels and a half. The kind of wheat used was the Kentish red, which was well pickled; and the work was finished on the 25th of September. That portion drilled with the widest intervals was the soonest overrun with weeds, but these were eradicated by horse-hoeing two or three times during the winter, and all the weeds entirely destroyed in April by the horse-hoe and by hand-weeding the rows; at the same time also the eight-inch rows were hand-hoed, but the broadcast was suffered to remain without weeding, from an opinion that wheat, when broken down, will never rise straight again, and that upon crooked and knotty stalks the ears are always inferior, and upon some totally barren. About a month afterwards the wide-rowed wheat had tillered most wonderfully, and had obliterated all traces of the rows; and this, as well as both the other portions, exhibited an uniform broadcast appearance, which was equally preserved in all till harvest; the whole three acres had indeed a rusty appearance on the stalks and chaff, but there was no difference in that respect; the broadcast was laid a good deal, the narrow rows in part, but the broad rows not at all; it was all reaped and shocked at the same time. In harvesting, each portion was separated with much exactness, and separately threshed: the broadcast produced twenty bushels of clean wheat, and three bushels of tail, or infe-

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rior; the eight-inch rows twenty-three bushels of clean, and one bushel of tail; and the eighteen-inch rows twenty-two bushels three pecks and a half of clean, and two bushels of tail;—the sample of the broadcast was thought full ten shillings a quarter inferior to the other; the narrow rows were a fair sample, but the wide ones far superior. The stubble of the wide-rowed acre was equal in cleanness and appearance to a garden, but that of the two other acres was fairly smothered with weeds and grasses.

Observations.—This experiment seems to have been made with every necessary degree of attention and accuracy, and indicates very plainly the superiority of the wide-drilled rows; the state of the stubble, however, is not entitled to all the importance given it by the writer, for he seems to have forgotten the additional labour and expense bestowed in weeding that part of the field.

Comparative Experiment with Spring and Autumnal Wheat, Smut, various Observations, &c. By AGRICOLA NORTHUMBRIENSIS. —*Agricult. Mag. No.* 28.

THIS very desultory paper merely gives it as the opinion of the writer, which opinion is founded on a single imperfect experiment, that crops of white Kent wheat and potatoe oats are each superior in value to a crop of spring wheat, both as to corn and straw. It is also stated that clean wheat, rubbed with the powder of smut-balls, and sown without any pickling, produced 63 smutty ears out of 99; while the same wheat, rubbed in a similar manner, and afterwards pickled in urine and lime, produced six smutty ears out of 90. The observations are *various* indeed, for they are connected with almost all agricultural subjects, without affording information on any.

Observations.—The experiment on spring wheat was too imperfect to be entitled to credit, in a comparative view, against autumnal wheat; but the trials with smut confirm Mr. Batchelor's opinion (*see page* 388) of the great utility of pickles for destroying smut, and place urine and lime in the same eligible point of view.

On the Culture of Parsnips in the Island of Jersey. By J. T. *of Patrimoine Cottage.—Agricult. Mag. No.* 29.

THIS vegetable is stated to be capable of feeding pigs in much less time than any other, and to be of general utility as a cattle-food; but the writer admits that these opinions are given on hearsay evidence, and that he has not verified the information by experiment.

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NATURAL PHILOSOPHY, ARTS, AND MANUFACTURES.

An Examination of the Review, which appeared in the thirteenth Number of THE RETROSPECT, *of Dr. Herschel's Essay on the coloured Rings discovered by Sir Isaac Newton.—Phil. Mag. No.* 139.

THIS paper is said to be transmitted "by a little club of friends, who occasionally meet together for conversing on philosophical subjects;" and who, professing high admiration of Dr. Herschel's "subtile and elaborate performance," were much surprised that it was but slightly estimated by the author of an article which appeared in The Retrospect. Our manner of criticizing the essay, "when confronted with that ample store of compliment and encomiums which usher in the strictures, is in some danger," they "fear, of being viewed by many as a very gross inconsistency; and so far, perhaps," they continue, "the reviewer has been unfortunate, in exposing the cordiality of his professions to some degree of doubt." Our mode of proceeding in our observations is called "a right cunning experiment, and by all the world like applying a metaphysical syphon to the merit of an author; by which, in the first instance, the merit is indeed made to mount up, but as certainly to wheel round to the contrary direction, till, by degrees, the whole stock should be drawn off, in the way of hocus-pocus, by the preponderant bias in the longer leg. But to interpose a little in favour of the reviewer," they add, "it is only fair to consider, that—he lay under a conflict between his great respect for Dr. Herschel, and his paramount obligation to fidelity, when delivering his awards in philosophical matters, for the edification of the public. It comes therefore, in the next place, to be examined how far the solidity of this reviewer's matter can justify the fervour of his critical zeal."

We had objected to Dr. Herschel's reflected rings, as being confessedly repetitions of those which Newton observed in a simpler form, and had asserted, that from this complication Dr. Herschel had obtained, in Horace's words, only *fumum ex fulgore*. The "club" is of opinion, that we should have thought Hadley "very idly employed in having to do with the moon and stars by means of reflections, when to be seen at a single glance in a simpler form:" they observe, that in a telescope we see much further and much better than "by looking at the same things in their simpler form;" that Archimedes, when investigating several curious properties of the sphere, "doubtless employed many reflections;" and that "the reviewer might impugn the demonstrations of that

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geometrician as unnecessary complications, with as much justice as he has done Dr. Herschel's proofs proceeding from reflections also, both mental and optical." They then propose the following questions:

"1. Considering the avowed importance of the subject, is it not an addition to our stock of knowledge to have been made acquainted with many different ways of obtaining a sight of these coloured rings, not only by means of glasses, but of metals?

"2. Is it not an addition to our stock of knowledge to have discovered and shown, that the colours of these different sets of rings are alternate, and to have explained the cause of that alternation?

"3. Is it not an addition to our stock of knowledge to have shown that the colour of the rings, as well as their size, can be suddenly changed, according to an invariable law, by the interposition of shadows, and to have explained the cause of these changes?

"4. Is it not an addition to our stock of knowledge to have proved also, by experiments, the four propositions in pages 47 and 48 *of the Essay*" [in our copy of the *Transactions*, pages 224 and 225], "relating to the action of the several surfaces as contributing to the formation of the coloured rings?

"5. Is it not an addition to our stock of knowledge to have shown, that from a polished metalline speculum we can obtain most beautiful coloured irises?

"If the reviewer still continues his negative to these questions," they observe, "his notions of knowledge and science must be of some very peculiar conformation."

The club in the next place defends Dr. Herschel for confining himself to the study of the works of Newton, because he was investigating the cause of the coloured rings discovered by Newton; and asserts, that although the phenomena of coloured thin plates were not the discoveries of Newton, yet "the coloured rings were discovered by Newton;" adding, "this, it is well known, Newton did by making use of prisms, and at last object-glasses brought into contact." They reprove us for having hinted, as they suppose, that Dr. Herschel was ignorant that light was liable to modifications at the surfaces of transparent bodies; and lastly, they enter into a calculation of the thickness of the wedge of air employed by Dr. Herschel, which, they maintain, must have been sufficiently small to have exhibited the colours of thin plates in a very conspicuous manner, at any point within three inches of its edge. The reason of the limitation of the number of rings, usually visible, to eight or ten, which we had cursorily noticed as being well understood, appears to them "altogether unintelligible;" and they seem to consider it as belonging to that class of doctrines which they are pleased to denominate abortive and fabulous opinions. They conclude with a most just panegyric on the merits of Newton. "That truly great

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and excellent person," they very properly observe, "had far more wisdom, as well as humility, than to harbour the weak and preposterous ambition of being accounted infallible as a MORTAL."

Why then should Dr. Herschel, or *his friends*, be disposed to complain of an individual, who, when obliged to declare his sentiments with sincerity, has thought it necessary to pronounce Dr. Herschel too a fallible mortal? We say Dr. Herschel's friends, because the writer of the observations has evidently quoted from one of the few separate copies which are usually printed, in such cases, for the author's distribution among his friends; and this we say not as a reproach, but rather as an apology for devoting more of our time to the examination of these remarks, than we should otherwise have thought necessary: and we trust that we shall be able to make it perfectly obvious, that our "paramount obligation to fidelity" did, in fact, as Dr. Herschel's friends think it "fair to consider," actually subject us to the disagreeable "conflict" between respect and disapprobation, which appears to them so inconceivable.

In such a conflict there is, in reality, nothing uncommon: we will not adduce examples from the cultivators of science, for fear of involving doubtful opinions; but was there ever a reader so void of feeling, as not to be filled with enthusiastic admiration and profound veneration, when he has been feasting on the banquet of eloquence which is displayed in the Orations of Cicero? and was there ever an individual so void of taste, as not to smile at the great orator, when he attempts to become a poet, and when, in defiance of the critics and reviewers of the day, he courageously *repeats* his verses, and insists that they are excellent? *Antoni gladios potuit contemnere si sic omnia dixisset*; and Dr. Herschel would probably never have been subjected to any animadversions from *our pens*, if he had written nothing but essays on coloured rings.

If Hadley, or Newton, or Hooke, or whoever invented the reflecting quadrant, had seen nothing by the assistance of the instrument, which they could not have seen *just as well* without it, we do confess that we should have thought them "*idly employed.*" A telescope is good *when* it shows us things further and better than they can be seen without it, otherwise it is *useless*; and all Archimedes's reflections, "both mental and optical," which we suppose to be intended as a specimen of Horne Tooke's metaphysics, would appear to us to have been worse than *puerile*, if they had only informed us that a sphere was round, and that it was less than the circumscribed cylinder, which the eye and the mind could at once have determined *without any reflection at all.*

We must now plead guilty to the charge of having, according to the ideas of Dr. Herschel's friends, some "notions of knowledge and science which are of a very peculiar conformation;" for we do continue our "negative" to all the questions which they

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have proposed. We think none of the circumstances mentioned of importance, because none of them are new. They might perhaps have been new to Dr. Herschel's friends, and were probably new to himself; but we will endeavour to show, that they can scarcely be allowed to possess enough of novelty to entitle them to a place in the Philosophical Transactions.

1. The "different ways of obtaining a sight of these rings," which Dr. Herschel has discovered, depend confessedly on more or less frequent reflections of the colours observed by Newton. If we place any object before a reflecting surface, we naturally expect to see it reflected, and this happened to the rings in Dr. Herschel's experiments. The mode of observation may, perhaps, be convenient for some particular purposes, but it does not yet appear *to what purpose* Dr. Herschel has applied it. We may, however, observe, that, for a conspicuous and beautiful exhibition of these colours on a large scale, no method with which we are acquainted is at all comparable to the solar microscope. That metallic surfaces would produce the colours as well as surfaces of glass, is no new discovery of Dr. Herschel: it has long been known, that the thin coat of litharge, which is formed by degrees on the surface of melted lead, affords them in a very brilliant form; and that the surfaces of other metals, when covered with similar pellicles, exhibit similar appearances; and we recollect to have heard it mentioned many years ago in a public lecture, and mentioned not as a new discovery, that a surface of glass, laid in a surface of polished metal, produced rings, in the same manner as two surfaces of glass. Dr. Herschel says, that the production of colours, in cases where a part of the rays is supposed to be absorbed by the metal instead of being transmitted, ought at least "to have been made a part of the hypothesis" of Newton; but it does not appear that this admission would have made any material difference in Newton's own conception of his theory; for, in the second book of his Optics, part iv. obs. 8, he asserts, "that the fits, or alternate dispositions of the rays, to be reflected and transmitted, are propagated to great distances from *every reflecting* and refracting surface;" and, in another part of the same observation, he speaks of "the refracting *or reflecting* surface which caused those fits."

2. We do not think it any "addition to our stock of knowledge" to have shown, that the colours of the "different sets of rings are alternate;" because Newton discovered, as is well known, that the colours, seen by transmitted and reflected light, are *always* alternate, or supplementary to each other: and Dr. Herschel has only attempted to investigate which of the rings that he saw were of one, and which of the other, kind; further than this he has not pretended to explain "the cause of that alternation." We allow him the merit of untying a knot, which he had before amused himself with tying.

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3. We doubt of the accuracy of the expression, that the colour of the rings, as well as their size, can be *changed* by the interposition of shadows: it is calculated to convey an idea of some mysterious "law" hitherto unknown; in fact, one series of the rings was *obliterated* by the interception of the light which afforded it, and another series appeared *in its place*, which was produced, according to the well-known laws of these colours, by another portion of light coming in a different direction. The shadows have only assisted Dr. Herschel, and the "little club of friends," in descending the hill which they had just before ascended, without having seen any prospect from its summit which was not visible from the plain.

4. The four propositions, which Dr. Herschel's friends found in pages 47 and 48 of the unpublished copy, and which stand in pages 224 and 225 of the Transactions, are these: "I. That only two of the surfaces are essential to the formation of the concentric rings. II. That these two must be of a certain regular construction, and so as to form a central contact. III. That the rays from one side or the other must either pass through the point of contact, or through one of the surfaces about the same point to the other, to be reflected from it. IV. And that in all these cases a set of rings will be formed, having their common centre in the place where the two surfaces touch each other." In these propositions we are utterly at a loss to discover any thing approaching to novelty, unless we understand the second of them in a sense which seems to be *no longer* intended by the author: but it is not our business to inquire what his opinions may at one time have been; we must confine ourselves to the experiments as they are published in the Transactions. No person, as far as we recollect, has ever imagined that more or less than *two* surfaces were concerned in these colours: Dr. Hook, who first described them, observes, that there must be a reflection at the second surface, and that "it is most evident that the reflection from the under or further side of the body is the principal cause of the production of these colours." If the rings are to be circular, the surfaces must be of a regular form, and if they touch, the point of contact will be the common centre of the rings: all this is undeniable, but certainly not new.

5. "That from a metalline speculum we can obtain most beautiful coloured irises," has been long since proved by the experiments of the Duc de Chaulnes, published in the Memoirs of the Parisian Academy for 1755. The Duc de Chaulnes used a plate of talc and a piece of gauze; instead of these Dr. Herschel employed the particles of hair-powder; and he has not attempted to show that hair-powder possesses any peculiar property not common to other opake or semiopake substances.

We trust, therefore, that we shall be excused, on these grounds, for still maintaining our absolute "negative" to all the questions proposed by Dr. Herschel's friends.

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If Dr. Herschel has confined himself to the study of Newton's works, and confuted him, he may have done enough for his own satisfaction; but an author, who offers his essay for publication, through the medium of a learned society, owes it to that society to inquire whether his researches are as original as they are important; for without such an inquiry we may have new essays on coloured rings, and new confutations of Newton, from different philosophers, in annual succession, and all on the same grounds: and as to the original discovery of the coloured rings, Dr. Herschel's friends would have done more wisely, if they had looked into the works of the author whom we mentioned, before they persisted in the assertion that "the coloured rings were *discovered* by Newton." We shall, however, now save them that trouble, by quoting some passages from the ninth chapter of Dr. Hook's Micographia.

"The laminæ of Muscovy glass, when reduced to a certain thickness, are coloured;—the same may be seen with looking-glass plates pressed together—even if wet with water—but then more faintly—with glass blown thin—with soap-bubbles—with bubbles of pitch, resin—and mucilaginous substances. The colours on steel, which has been heated, are produced by a vitrified lamina—and on other metals—especially lead. The body must be of a determinate thickness—and it must be transparent.—If there be a thin transparent body, of a thickness gradually increasing, these shall be yellow, red, purple, blue, green, yellow, red, purple, blue, green, and so forth—so often repeated" as, according to his hypothesis of colours, the thickness of the plate allows. The colours visible in soap-*bubbles*, and in *bubbles* of pitch and resin, are *concentric rings:* the contact of *looking-glasses* also often produces rings; and, at any rate, it follows immediately from Hook's words, that where there are rings of a transparent substance *of a determinate thickness*, as between the object-glasses, there must be coloured rings. How then can it possibly be asserted that Newton discovered even the *rings* of colours?

Dr. Herschel's friends have much misunderstood what we said respecting the modifications of light: we should not have imagined that there was any ambiguity in our expressions. We said, that, "because the rays of light undergo certain flections and modifications in the neighbourhood of bodies near which they pass, it surely does not follow that they are not liable to *other* modifications at the surfaces of transparent bodies;" meaning the Newtonian fits of reflection and transmission. Dr. Herschel seems to suppose that the modifications are *similar* in both cases; and his friends have understood us as if, instead of *other*, we had said *any* modifications at all.

We observed, that the colours produced by the hair-powder were identical with those of the coronæ described by Mr. Jordan

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and other authors. They are at least much more analogous to these coronæ, in the mode of their exhibition, than to the colours of *thin plates*; and this is the only point of our remark which bears on the question. It has been supposed by some theorists, that the colours of coronæ are always dependent on a collection of particles nearly equal in magnitude; and it is doubtful whether such an assemblage of particles can be found in hair-powder. There is another mode of explaining them, which we shall not minutely examine at present, but shall only observe, that a principle has been laid down, in virtue of which a particle, placed before a concave mirror, may be expected to produce the same fringes of colours, as would surround a shadow of the particle, projected by a luminous point, situated in the virtual focus of the mirror conjugate to the place of the particle. Had Dr. Herschel taken any measures of the rings, we could easily have compared this supposition with the result of his experiments.

We now come to the last and the most plausible objection that Dr. Herschel's friends have advanced to our remarks; and here we grant, that such a wedge as they have described ought to have produced the effects which Dr. Herschel expected from it: but it is equally certain, that such a wedge would actually have exhibited its original stripes of colours near its edge, and these colours Dr. Herschel obviously did not observe; if he had observed them, he could not have failed to describe the singular appearance which the rings would have presented in crossing their dark spaces. The fact is, that no such wedge can be obtained, without extreme care in employing perfectly plane surfaces, extremely free from dust, rubbed together with great force, and retained in their relative situations by a considerable pressure; and it is well known to those who are conversant in such experiments, that the *only test* of the actual contact of the substances employed is the appearance of the central dark space, and of the various alternations of colours which succeed it. Another of Dr. Herschel's wedges his friends have tacitly abandoned, without attempting to reconcile it with the Newtonian measures.

The "counteractions" which have so much puzzled Dr. Herschel's friends, are nothing more than the mixture of various colours, which produce white light, and so far counteract each other's tendency to produce rings of any given colour. If any spot be taken beyond the first ten or twenty rings, it will appear, from Newton's measures and calculations, that so many rings of different colours will coincide in this spot, that none of them will prevail, and the general effect will be white. This fact is too notorious to require "an argument;" and it is singular that our opponents should have imagined that we meant to argue on such a subject. We do not, however, regret having an opportunity of stating this fact more distinctly, because Newton seems in some measure to have lost sight of it in his explanation of the colours

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of thick plates: nor do we lament the necessity of making some reply to the remarks of a number of individuals, however small, who have at least the merit of taking an interest in the investigation of these subjects, since they justify us in having entered into a more full elucidation of the progress of this department of science, than we should otherwise have thought it eligible to undertake; and since that which is now advanced, merely in self-defence, might, if not so imperiously demanded, have been deemed an officious and an invidious interference.

Continuation of Experiments for investigating the Cause of coloured concentric Rings, and other Appearances of a similar Nature. By WILLIAM HERSCHEL, *LL.D. F.R.S.—Phil. Trans. Part II.* 1809.

THIS paper is the sequel of the essay which has been the subject of the preceding discussion. We shall proceed to lay before our readers a short account of its contents; and we shall be under the necessity of adding some remarks, which we fear will again subject us to the displeasure of those gentlemen whose censures we have been endeavouring to confute.

In the first article of these observations, which is numbered as the 35th of the whole, Dr. Herschel shows, that "cylindrical curves produce streaks." One surface of a plate of glass being ground to a cylindrical form, it exhibited, when in contact with a plain surface, streaks of colours, which "differed in no respect from rings, except in their linear instead of circular arrangement." The next two articles inform us, that "cylindrical and spherical surfaces, combined, produce coloured elliptical rings;" and that "irregular curves produce irregular figures." The latter proposition is illustrated by experiments on plates of mica, brought into contact with glass by different degrees of pressure, which occasioned continual changes in the colours. In the 38th article it is asserted, that "curved surfaces are required for producing the coloured appearances at present under consideration;" for, as the author proceeds in the 39th, "coloured appearances cannot be produced between the plain surfaces of two parallel pieces of glass applied to one another." For a proof in the case, when the plates are a little inclined to each other, the author refers to the former part of his essay: he says, that he has seen two plates of glass separated at one end by a thin piece of platina, and tied together at the other, "which showed some streaks near the place where the glasses were in contact; but when he removed the thread that bound them together, the streaks vanished; which proves, that the glasses had been constrained, and thus had *probably* assumed some curvature at the point of contact." He has also tried to produce colours by pressing together two flat surfaces of glass, but they

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showed only "large flashy coloured appearances," arising from the change of figure produced by the pressure. He says, however, in concluding the article, that "when the incumbent plain glass is not *of a parallel thickness*, coloured phenomena may be rendered visible between two perfectly plain surfaces," without any "force or strain." "Hitherto," he continues in the 40th article, "I have only considered the coloured rings which Sir Isaac Newton has pointed out, and have shown, that no more than two surfaces are essential to their formation." This article, and the three following, are devoted to a minute description of the coloured bows which constitute the limit between total and partial reflection in the surface of a prism, as viewed by an eye placed at a moderate distance. He observes, that the blue bow contains some green, followed by blue, indigo, and violet; and that a very faint red, orange, and yellow, may also be perceived above the greenish colour. He requires us, in the next place, to "admit, that the colours, which give it the general appearance of what may be called blue, consist of half the green, and of all the blue, indigo, and violet rays, which are reflected, while the other half of the green, the yellow, orange, and red, are transmitted:" and upon this supposition he proceeds, employing the Newtonian measures of the infrangibility of the different rays, to determine the dimensions of the different portions of the bow; making its breadth only 21′ 41.5″, and observing that Newton is mistaken in calling it 1° 4′ 31.4″. The angles, at which the rays that constitute the blue bow are separated from the rest, Dr. Herschel calls *critical*, and the effect a critical separation. The red bow, which appears by transmitted light, he seems to consider as having been hitherto unnoticed: he calculates its dimensions, and makes its breadth 15′ 46.1″ only; he then proceeds to show how the bows may be rendered visible by being projected on a white ground. The blue bow, he observes, may be instantly transformed into a red one, by changing the direction of the light which renders it visible; and, by proper management, we may have the bow half red and half blue. "According to my account of the red bow," continues Dr. Herschel, "it ought to be seen in the prism *a little* above the blue one, and this is also further *confirmed* by any one of the experiments, in which we have some part of each bow in view at the same time." In article 44, Dr. Herschel describes the streaks and other phenomena, which, as he thinks, are "produced from the prismatic blue and red bows." "When a plain glass or metalline mirror is laid under the base of a right-angled prism, in which we see the blue bow, the contact of the two plain surfaces will immediately produce a great number of coloured streaks. They will be found to be parallel to the bow, most of them within, and some just under it. They may be seen *without any lens*, merely by looking into the prism, with the eye pretty close to the surface through which we see the blue bow. This experiment

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proves," in the author's opinion, "that plain surfaces, though they cannot produce colours, have a power of modifying and multiplying them when produced." An object-glass, placed under the prism, affords us a great variety of concentric coloured rings. Similar phenomena are observed when these surfaces are applied to the red bow; and the application of a flat plate of glass is a good test, for distinguishing the bows formed by the critical separation from any other coloured images. The different appearances of the prismatic bows, in prisms of different forms, and according to the different directions of the light, are described in article 45. We are next informed, that "the first surface of a prism is not concerned in the formation of the blue bow, nor of the streaks that are produced by a plain glass applied to the efficient surface:" and in article 47 it is observed, that "the streaks which may be seen in the blue bow contain the colours of both the parts of the prismatic spectrum, by the critical separation of which the bow is formed;" and the alternations of these colours are enumerated, to the number of 13 or 14; some of the rays which have been transmitted re-entering the prism by reflection. Dr. Herschel attempts to illustrate "the formation of streaks" in the next article; and three diagrams are exhibited, in order to show the course of the rays, on a scale a thousand times magnified, through a flat plate of air one ten thousandth part of an inch in thickness. From these figures Dr. Herschel thinks it evident, that "streaky appearances must be produced." Between the utmost limit of the bow, "and *the first* red ray which re-enters the base, a narrow blue streak should be seen. When we advance further into the figure, the great mixture of the colours, and the different directions of the rays, are so various, that *nothing particular* can be determined without entering into a very complicated calculation of the meeting and intersections of the rays; we see, however, that these mixtures will produce a condensation of rays in some parts, and vacancies in others, so that no uniform tinge can remain, and consequently *streaky appearances* must be seen." "Towards the violet end of the bow, streaks approaching to white will appear, such as pale red, pale bluish green, dingy yellow, and dirty white; so that, both at the beginning and end of the bow-streaks, all observations of them agree perfectly with what is pointed out by the foregoing remarks; and though we have not analysed the particular construction of the streaks in the middle of the bow, yet what has been said will sufficiently prove that various successive changes of the colours must also take place." The 49th article informs us, that "prismatic bows, when seen at a distance, are straight lines." The 50th contains one of the most material propositions in the whole paper, that "the colours of the bow-streaks owe their production to the principle of the critical separation of the different parts of the prismatic spectrum."—"Let a plain glass be laid under the base of a right-angled prism; then, if the eye

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at first be placed very low, no streaks will be seen, but when afterwards the eye is gradually elevated, till by the appearance of the blue bow we find that the principle of the critical separation of colours is exerted, the streaks will become visible, and not before; nor will they remain in view when the eye is lifted higher than the situation in which the effects of the critical separation are visible. It is therefore evident," says Dr. Herschel, " not only that the colours are furnished by the same cause which produces the bow, but also that they are modified into streaks by the plain surface under the prism." " When a spherical surface is placed under the prism, it has likewise been shown that coloured rings will be seen; but these, like the streaks, will not be visible when the eye is below the place where the bows can be seen, which would not have happened had a plain glass been used instead of the prism; for, with such an arrangement, coloured rings may be seen at the most oblique as well as *perpendicular* stations of the eye.—The eye being gradually elevated, these rings will shrink; till at last, when the eye comes to a *perpendicular* situation, we find them dwindled away to the size and appearance of such as may be seen when a plain glass is substituted for the prism." In order to extend this theory of the effects of a prism to lenses, it is asserted, in the next article, that " a lens may be looked upon as a prism bent round in a circular form;" and the analogy is illustrated by grinding a small part of a plano-convex lens flat, that the author " might look into it, as it were, through a window, to see what passed within: the flat made an angle with the base of about *thirty-four degrees*," and he saw *through* it the blue and the red bow. When two plain glasses are laid together, Dr. Herschel seems to intimate that colours are formed, though streaks cannot be seen; the rays, which would exhibit them, not being able to " come out of the glass." The 52d article bears for its inscription, that " the critical separation of the colours, which takes place at certain angles of incidence, is the primary cause of the Newtonian coloured rings between object-glasses." Having shown, that in all prismatic experiments the colours are exclusively produced by the critical separation of the rays of light, he says, that it certainly will not be philosophical to look for a different cause of the same or similar effects, when convex glasses, which have all the required prismatic properties, are used to produce them; observing, however, in a note, " that if any case should occur, in which the critical separation cannot account for the observed phenomena, we are then authorised to look out for some other cause to explain them." He compares the colours afforded by convex glasses and by prisms, and very naturally concludes that they must be identical, and that they must therefore be derived from the same cause, which he maintains to be the critical separation of the colours. In the 53d and concluding article Dr. Herschel remarks, that since all these phe-

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nomena may be explained from the "Newtonian different refrangibility of light," "we can have no longer any reason to ascribe original fits to the rays of light, which, in the first part of this paper," he says, "they have already been proved not to possess, and which now, in all prismatic experiments," he has shown, as he imagines, not to be "necessary for explaining appearances, that may be accounted for without them."

Observations.—It is with sincere and unaffected sorrow that we are now about to perform the disagreeable task of pointing out in the present paper, several inaccuracies, which appear to us even more objectionable than any which we noticed in the former. If we are mistaken, we shall most willingly be convinced; but we hope that Dr. Herschel's friends will as readily forgive us for expressing our opinions, as we forgive them for attempting to confute us.

Against the three first sections we have nothing to object: they contain nothing that is new, but for this we are prepared. The two next articles are occupied by the attempt to prove that curved surfaces are necessary for producing the coloured appearances under consideration; and that coloured appearances cannot be produced between the plane surfaces of two parallel pieces of glass; but the author adds, when the incumbent plane glass is *not of a parallel thickness*, coloured phenomena may be made apparent between two perfectly plane surfaces; and yet in the same page he asserts, that no more than *two surfaces* are essential to the formation of coloured rings. Where there is so much apparent contradiction in expressions, it is generally as useless as it is difficult to search for an author's true meaning. We cannot suppose that the coloured appearances are considered by Dr. Herschel as being of a different nature from the coloured rings, since he expressly ascribes them to the same origin: he seems, however, to be of opinion that *either* curved surfaces, or prismatic forms, are required for the production of colours: but it is remarkable that in his theoretical investigations he has completely omitted the consideration of the curvature of the surface; except where he compares a lens to a bent prism; forgetting, in this comparison, that one of the surfaces of the lens had before been proved to be unessential to the effects. His explanation of the disappearance of the colours between the two pieces of glass, separated at one end by a slip of platina, is wholly arbitrary and forced, nor can it be admitted, until he shows that the same colours are not produced by a pressure immediately upon the point of contact. It is surprising that he should have imagined that he had a right to expect the appearance of colours on the Newtonian principles after the removal of the thread: the mutual repulsion of the glasses was observed by Newton himself; Professor Robison made it the foundation of some very interesting experiments on the

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elementary properties of matter, and it has not altogether escaped Dr. Herschel's own observation. A late author, in a description of an apparatus for exhibiting these colours, published about seven years ago, has observed, that "the two glasses must be retained in their position by means of three screws; for, *as soon as the pressure is removed*, they repel each other with considerable force; and, for this reason, neither of them ought to be very thin, otherwise they will bend before they are sufficiently near." Besides, when a little oil has been suffered to spread on the surface of water, or when a thin coat of litharge is formed on lead, the uniformity of the colour, for a considerable extent, undeniably indicates that the surfaces are parallel; and that they are perfect planes, the laws of hydrostatics are sufficient to prove. We must therefore beg leave to express our firm persuasion, that the curvature of the surfaces concerned is *not* essential to the production of any of these colours, any further than as affecting the thickness of the plate, and thus regulating their succession.

We must, in the next place, make several objections to the descriptions of the blue and red bows, contained in the four following articles. It is true that the blue bow contains "some green, followed by blue, indigo, and violet;" it is also literally true, that "a very faint red, orange, and yellow may also be perceived above the greenish colour:" but it is equally true, that whenever they are so perceived, there is an optical fallacy, which has deceived others, as well as Dr. Herschel. If we fix our eye intently on the blue bow, and then look a little above it, a reddish tint is very manifestly observable: but this tint is equally visible on any other part of the white surface to which we immediately turn our eye; nor is it discernible when we look down gradually more and more, descending through the white light, till we arrive at the bow; so that the appearance of the red above the blue can only be referred to that class of phenomena, which have been denominated ocular spectra. We do not wish to rest this statement on our own testimony only, but it is the unavoidable consequence of the Newtonian doctrine respecting the refrangibility and reflexibility of light, which Dr. Herschel fully admits. Above the bow, the whole of the colours which constitute white light are reflected to the eye; at its upper edge, the least refrangible rays begin in great measure to be transmitted, leaving the rest of the rays to proceed together to the eye, forming a tint of that kind, which is produced by subtracting the extreme red rays from white light, that is, a bluish green, and such must be the colour of the upper edge of the bow. In Newton's own words, substituting only for the references to the figure, "all the least refrangible rays which fall above the bow, and can come from thence to the eye, will be reflected thither; but at all points below the upper edge of the bow, many of these rays will be transmitted through the base" of the prism, and every where within

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the limits of the bow, where the more refrangible rays are reflected, and the less refrangible are transmitted, "the *excess* of the most refrangible in the reflected light, will tinge that light with their colour, which is violet and blue." No part of the reflecting surface can possibly be tinged red or orange, unless we suppose the least refrangible rays to be reflected in greater abundance than the most refrangible, which is contrary to the acknowledged principles of the calculation. It is true that Newton has committed a mistake in the magnitude of the angles concerned: the ratios of the sines, for the extreme rays, being those of 154 and 156 to 100, the angles, of which the natural secants are 1.54 and 1.56, are found, by an inspection of the tables, to be 49° 30.4′ and 50° 7.9′, of which the difference, 37.5′, is the true breadth of the bow. The red bow is merely the converse or the supplement of the blue one; its breadth must therefore be the same, and it must appear at the same elevation of the eye. It might easily appear to Dr. Herschel a little more elevated, because the commencement or upper edge of the blue bow differs but little from white light, and is much less strongly marked than the corresponding sharp termination of the red one. By covering with two pieces of paper the opposite halves of two of the sides of a common prism, and looking up through the third towards the sky, we may obtain a perfect view of both bows, placed side by side, and equal in breadth; while, according to Dr. Herschel's calculations, the red bow ought to be not only a little, but completely, above the blue one. The red bow is particularly described in Newton's Lectiones Opticæ, and the situation of the eye, in viewing it, is correctly represented in the 49th figure of that work, pl. VIII. p. 260. If we thought Dr. Herschel or his friends would condescend to be indebted to the labours of any of their contemporaries, we could refer them to a publication, in which all this is very clearly illustrated by a coloured diagram; but we hope they will at least acquiesce in our explanation of Newton's own statement. Dr. Herschel has not even thought it necessary to pay the least attention to any modern remarks respecting the division of the spectrum; but speaks of orange and indigo *rays*, as if they actually ought to occupy the spaces, which Newton, or Newton's assistant, assigned to them.

The streaks, parallel to the bow, which are described in the 44th article, might easily puzzle a person not acquainted with the extent of the Newtonian discoveries respecting the colours of thin plates; but they are the immediate and necessary consequence of the law, which Newton has laid down, for calculating the effects of light falling obliquely on a thin plate. The glass or mirror is held, by the repulsive force, and in general still more by the inequalities of the surface, at a considerable distance from the prism; but Newton has shown, that an increase of the obliquity of the rays, passing through a thin plate, has the same effect as a

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diminution of its thickness, and this almost without any limit: now it is obvious that this obliquity must increase continually in ascending towards the bow, till the light immediately next to the bow becomes, in its passage, ultimately parallel to the surfaces; and thus the same effect must be produced, as if the surfaces there were actually in contact, while at other points more remote from the bow, the varying degrees of obliquity must exhibit the same effects, as a plate of variable thickness, viewed by light falling on it in a direction nearly perpendicular. This effect not only *may* be seen without a lens, but must always be rendered *more indistinct by* a lens of any considerable aperture, because it comprehends rays differing very much in obliquity; while in other cases, a slight change of obliquity does not produce any material change of appearance. It is the great obliquity of the light, passing through the plate of air, that renders it so easy to exhibit colours in it by the application of a flat surface without pressure: hence the appearance of colours, in such circumstances, becomes very naturally a criterion of the obliquity, which necessarily accompanies what Dr. Herschel calls a critical separation.

The 45th, the 46th, and 47th articles require no particular notice. The gigantic diagrams of the 48th appear to us to prove absolutely nothing. The only argument, that we can discover, is contained in these words, omitting the letters of reference: "between the utmost limit of the bow and *the first red ray* which re-enters the base, a narrow blue streak should be seen." But there is, in fact, *no first* red ray; there are other red rays which re-enter the base on each side of that which Dr. Herschel, for some reason which we are at a loss to imagine, has chosen to *call* the first; and some of these other rays must unavoidably fill up the whole of the "narrow blue streak." Nor does the existence of any other part of the "streaky appearance" stand on a firmer foundation. We are also to observe that the whole of this supposed explanation is limited to the beginning and end of the bow, and at most to the interjacent space; that is, in the whole, to an angular extent of about one third of a degree, and to angles of obliquity between $49\frac{1}{2}°$ and $50\frac{1}{3}°$, within the glass. The experiment related in the 50th article, is a modification of the observation on the parallel bows, which we have already explained; they cannot be seen when the eye is very low, because the reflection is then total, and the surface of the prism may be considered as impermeable to light; and they will generally disappear when the eye is much elevated, because either a great obliquity, or a very intimate contact, is required for producing them: for it appears even from this article, that the rings, produced by the *closer* application of a spherical surface to the prism, were visible when the eye came "to a perpendicular situation." We have no objection to considering a lens as a prism bent round in a circular form, provided that the angle of the prism be always supposed equal to

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the mutual inclination of the surfaces of the lens at the points concerned: but when Dr. Herschel made "a window" in his planoconvex lens, by introducing a new surface, which formed an angle of thirty-four degrees with the base, he converted it at once into a prism, and he could no more see in this manner, what passed within a perfect lens, than the youth, who is said by an ancient author to have stood with his eyes shut before a mirror, could see how he looked when he was asleep. The bows are relative to the situation of the eye only, and their being seen through the "window" only proves that they existed with respect to an eye so situated; but the question is far too simple to require an experimental investigation. It is, however, remarkable, that a substance, terminated by parallel surfaces, however bent into a cylindrical or spherical form, will never exhibit bows of colours by a critical separation; the reflected and transmitted rays, in the neighbourhood of their common limit, proceed in the same direction, so that the limit cannot be distinguished; and if it were otherwise, it may be shown that the apparent place of that limit is independent of the refractive power of the substance; the first ray, which, after total reflection and refraction, emerges parallel to any given axis, being always the continuation of a tangent to the interior surface.

The title of the 52d article appears to contain the most material proposition of the whole paper, that "the critical separation of the colours, which takes place *at certain angles of incidence*, is the primary cause of the Newtonian coloured rings between object-glasses." As far as our recollection serves us, we have never met, in any work of science, with an oversight in reasoning so disproportionate to the acknowledged talents of the author, as that which is contained in this article. Dr. Herschel seems to be fully aware that a "critical separation" takes place only *at a certain angle*, that is, in glass about 50 degrees; he has himself confessedly seen that the coloured rings are visible *at all angles*, and yet he has the inconceivable faculty of satisfying himself, that the critical separation is the general cause of these colours! In thin plates of greater density than the surrounding medium, there can be *no* critical separation at *the first surface*, which is necessary in Dr. Herschel's theory, *whatever* may be the obliquity; and it has been observed by Newton, that such plates usually exhibit the colours even with greater brilliancy than those of a contrary description. These objections would be equally valid, if it had been proved that the critical separation is capable of producing bows, but we have already observed, that the supposed proof is altogether founded on an arbitrary and inaccurate mode of stating the question. If therefore the Newtonian fits of easy reflection and easy transmission were "necessary for explaining" these appearances, before the publication of Dr. Herschel's experiments and calculations, we cannot help thinking that they remain equally indispensable after all that he has written on the subject.

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Method of correcting the Damp in Coal-mines. By Dr. Jones.—*Monthly Mag. No.* 191.

Dr. Jones observes, that the damp in coal-mines is nothing but an excess of hydrogen gas; wherever it occurs, however, it is an afflicting circumstance, and the miners not unfrequently fall a sacrifice to its effects. The remedy which he proposes, is to slake a few lumps of fresh lime in the subterraneous passage made by the miners in digging out the coals. The carbonic acid gas evolved by this process corrects the air in the mine, and renders it more favourable to inhalation and combustion. This experiment was tried a short time since in the coal-mines belonging to Lord Cawdor, at Lanlash, in Carmarthenshire, which were annoyed with the damp, and found to answer the desired purpose.

Observations.—This is a subject of so much importance, and the fatal effects of this damp are so frequently experienced in this country, that every thing, which either theory or practice may suggest, for preventing them, merits attention and investigation. There are, however, two points which Dr. Jones does not seem to have sufficiently considered; first, whether any carbonic acid is emitted when lime is slaked; and secondly, how it is possible that carbonic acid should possess any power whatever of counteracting the effects of hydrogen.

On atmospheric Phenomena. By Mr. J. B. Van Mons.—*Phil. Journ. No.* 107.

The author of this communication states, that he had shown, in a paper which he laid before the Batavian Society of Experimental Philosophy at Rotterdam, that thunder-storms are wholly and spontaneously formed in the atmosphere. He conceives, that a diminution of solar and lunar attraction deprives the air of that additional elasticity which this attraction had imparted to it, and thus permits it to descend, which loosens the union between the air and water: the temperature is raised by the separation of the caloric which constituted the medium of this union, the water separates, and clouds are formed. These soon enlarge by a continuation of the same cause, and abundance of caloric is set free; but as the air is a bad conductor of heat, this is not easily diffused through it, nor can it be dissipated in the form of light, for want of sufficient concentration; it therefore becomes electric fluid, and decomposes the water of the cloud. That portion of caloric which in combination with air serves to convert water into permanent gas, is contained in the state of electric fluid, "or at least in

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a state intermediate either to that of heat and electricity, or of electricity and light; which fourth state being incapable of subsisting, except in combination, will never be known to us separately, or otherwise than by its effects. This state is the agent, by means of which permanent gasses retain their state. With the bases of these gasses it enters into a chemical union, which can be broken only by an affinity of the same nature." The author supposes, that a greater or less degree of elasticity constitutes all the difference between light, the electric fluid, sub-light, and sub-electricity (if indeed this exist), and heat (caloric); and remarks on the facility with which these agents are transformed one into another. It is only the caloric which is partially and copiously separated, which is thus transformed into electric fluid. The heat of the atmosphere he regards as spontaneous, and not communicated by the winds, which are the effects and not the causes of changes of temperature.

At every increase of atmospheric temperature the mercury in the barometer sinks, because the precipitation of water diminishes the elasticity of this fluid. A diminution of temperature, which is the result of a combination of water and air, a fixation of caloric, and a transformation of caloric into electricity, causes the barometer to rise by the increase of elasticity which it imparts to the air. Hence it is inferred, that the rain which falls while the barometer rises and the thermometer falls, is a rain of recomposition of the air in its aqueous combination; and that which falls while the barometer sinks and the thermometer rises, is a rain of decomposition of air with respect to that combination.

Mr. V. M. then remarks, that the rise of the barometer is caused by the elasticity of the air, and not by its weight. On this head he observes, "I cannot easily conceive how people continue to ascribe to the weight of the air that pressure which this fluid exerts on the mercury in the barometer; while we see it almost always increase this pressure, when it loses part of its matter or gravitating power; and diminish it when the air is at a *maximum* of aqueous saturation, or just before rain; and when the bulb manometer, or true aërostatic balance, indicates the ultimate degree of density in the air: and that all other phenomena, both those that occur in nature, and those that present themselves in experiments with the mercurial pump, prove to a demonstration that the air presses chiefly by its elastic power, which is increased by condensation, and by the addition of caloric, the matter remaining the same in a closed space; and diminished by rarefaction, and the subtraction of caloric; the matter remaining equally the same, and in the same space; but which is neither increased nor diminished in the open air, but by the association, more or less elastic, more or less solid, of water with air." From this he concludes, that experiments made in confined air are not applicable to meteorology.

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Some farther remarks are next offered on the formation of clouds, and their becoming charged with electric fluid; and it is asserted, that the gasses of water dissolve in the air as spirit of wine dissolves in water, and remain diffused in the matter of the cloud. This state, however, is soon destroyed, either by the condensation of the fluid or the intensity of the charge; and the water is recomposed by the inflammation of its gasses. The flashes of lightning, which are nearly without noise, are the effects of the explosion of the electric spark; and those which are accompanied with thunder, are those of the combustion of the gasses of water; these sorts of lightning succeed each other alternately. The rolling of thunder is occasioned by a succession of partial inflammations. Thunder is also of two kinds, the sounds of which are different; that of the fulgurations being acute, quick, and snapping; and that of the fulminations heavy, dull, and rolling. The sound of thunder, which bursts towards the earth, is very different too from that of rolling thunder, and is distinguished by the appellation of falling thunder. When the greater part of the water of a storm is dispersed, the remaining electric fluid, not finding any thing to which it can adhere, is concentrated into a point, and darts towards some prominent points on the surface of the globe. The great destruction which lightning sometimes commits arises from the difficulty it finds in resuming an equilibrium, or of diffusing or decomposing itself. A decrease in the temperature of the air presages a cessation of its stormy state, while either an increase of temperature, or a continuation at a high degree, is always followed by a recommencement of the storm. The opposite winds during a thunder storm are the effects of a strong condensation of the aqueous part of the atmosphere.

It is then remarked, that a thunder-storm does not arise from an accumulation of hydrogen gas, as this does not ascend into the upper regions of the atmosphere. Neither is rain the consequence of aqueous vapour by cold, as it always precedes the cooling of the air, while an increase in the temperature of the air always precedes rain. It is also observed, that water forms almost a fourth part of the weight of the atmosphere: the fact and experiments on which this conclusion is founded are given in the paper alluded to at the beginning of this article. This writer remarks on the difficulty of establishing this point synthetically; and observes, that "the method which has succeeded best for this purpose, is the disengagement of muriatic gas from a very dry muriate, by means of highly concentrated sulphuric acid, in confined air."

Mr. Van Mons also states, that caloric is a substance which belongs only to the solar atmosphere, and that a perpetual circulation of light takes place between the sun and those globes which constitute a part of his system. Were it otherwise, the accumulation of caloric on the surface of these globes would soon change

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the nature of things, and our calculations relative to the magnitudes of the planets would be erroneous, answering to the extent of their atmospheres, and not to the celestial bodies themselves. It is to be understood also, that the presence of the sun is necessary for the attraction of this light from the planets, and that this extrication of light can only take place from those parts of their atmospheres which are opposite to the sun, and directly under his influence.

Observations.—It may not be amiss to state in this place the commonly received doctrine relative to the relation which subsists between the weight and elasticity of air. It is generally conceived, that the weight or pressure of the air does not at all depend upon its elasticity, and that it is neither more nor less heavy than it would be if it were a non-elastic substance: but its elasticity renders it susceptible of a pressure, which reduces it to such a space, that the reaction of its elasticity against the pressing force is exactly equal to that force. The law of elasticity also is such as to increase in proportion to the density of the air, and this density increases very nearly in proportion to the weight or force by which it is compressed: but as there is necessarily an equality between action and reaction, the gravity of the atmosphere which effects its compression, and its elasticity by which it tends towards expansion, must be equal.

We should have been glad to have been made acquainted with the experiments and facts which Mr. Mons states he has given in the paper to which he refers, and on which he rests the two following propositions: viz. "that thunder-storms form in the atmosphere spontaneously and wholly;" and that "the water of the atmosphere constitutes nearly one fourth of its weight." Until these assertions are fully established, we cannot help regretting, that on grounds apparently so slender, and so much in contradiction to the experiments of other philosophers, Mr. Van Mons should have wasted his ingenuity in raising a superstructure of mere hypothesis, which explains nothing that is not equally clear without it, and which is accompanied by no reasoning that can satisfy the mind of a cool inquirer.

On the Date of the Application of Telescopes to astronomical Instruments; the Time when Astronomers first observed the Stars and Planets in the Day-time by means of these Telescopes; and on the Author of these Discoveries. Translated from the French of M. De Fouchy.—*Phil. Mag. No.* 138.

The French Academy of Sciences was desirous of discovering the author of the application of telescopes instead of sights to astronomical instruments; to determine the date of this invention; and to ascertain the time when astronomers first observed the stars during the day-time: and to fix these points, the author of

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the present essay was deputed by that learned body. The one of these circumstances, to which astronomy is so much indebted for its present perfection, was generally attributed to M. Auzout, and the other to M. Picard. M. Fouchy at length found these points discussed in Morin's *Science des Longitudes*. In the celebrated conference which the author had on this subject with the commissioners appointed by Cardinal Richlieu, on the 30th of March 1634, he made use of the following expression when describing the means of preserving the accuracy of these instruments while their radii were diminished: "The third method is the application which I have found means of making of a telescope to the index instead of sights, to measure more readily and more accurately the distance of the moon from the stars." M. Fouchy has not been able to determine the precise date of this invention of Morin; but thinks there is great probability that it was not long prior to the above date. The astronomer to whom we are indebted for the application of telescopes to instruments of this nature, was also the first who observed the stars while the sun was above the horizon: this he effected about the end of March 1635.

Observations.—The application of telescopes to astronomical instruments, and the discovery that the planets and stars could be seen, by means of a telescope, in the day-time, are important events in the history of astronomy. By these not only the accuracy of observations was much increased, but also their time of continuance was greatly lengthened; and new views opened to the astonished astronomer. They likewise afforded the most certain means of establishing the elementary principles of astronomy; and consequently both their author, and the periods which gave them birth, are worthy of being determined with critical accuracy, and recorded in the archives of science. Notwithstanding the authority of the names of those who have either claimed the honour of these inventions, or have had it voluntarily attributed to them, we have no doubt that it is Morin who is so justly entitled to the gratitude of astronomers in this respect.

We ought not to quit this subject without assuring the translator of this paper, that we are far from being satisfied with his performance. As a specimen, we shall transcribe the two following sentences, which constitute a part of Morin's description of his first observing the planets and stars in the day-time, and we shall leave our readers to judge of the accuracy and elegance of the language. "Some time before daylight, whilst the bright stars were yet visible, I put Arcturus in my telescope, and kept it there almost until the rising of the sun; remarking, with pleasure, that I yet saw it distinctly in the telescope, a long time after the brightness of the day made it disappear to the naked eye: a cloud covered it at this moment, and when it was passed I could not, with every endeavour, find it again. This accident distressed me

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but I was patient, well knowing that had it not been for this cloud I should have been able to have seen the star much longer."

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1. *Meteorological Observations on a Thunder-storm; with some Remarks on medical Electricity. By Mr.* CORNELIUS VARLEY.—*Phil. Mag. No.* 137.
2. *Further Remarks on Thunder-storms. By the same.—Phil. Mag. ibid.*
3. *Account of some luminous Meteors seen during a Thunder-storm. By* JAMES STAVELEY, *Esq.—Phil. Journ. No.* 108.

1. MR. Varley states, that in this thunder-storm, which fell more immediately under his observation, the electrical phenomena were so distinctly visible, as to leave no doubt on his mind of the exclusive agency of one electric fluid. The horizon, towards the north, was lined with dark clouds, but there were none over head, except a very few light ones, proceeding towards that quarter, and which afterwards joined those seen in the horizon. This circumstance Mr. Varley regards as a proof that a diminution was then taking place in the volume of the atmosphere, otherwise these small clouds could not have overtaken the storm, being at first too far distant from it to be affected by any electrical attraction. The dark cloud was of a long form, and at first a small round cloud appeared behind, and was seen above it. In this cloud it frequently lightened; and immediately after the light was extinguished, electric sparks struck to the earth from the under part of the lower cloud. This twofold transmission was many times repeated; at length, however, the lower cloud ceased to emit sparks to the earth directly below the upper one, though it frequently received fresh supplies from it. Mr. V. then perceived that each charge from the upper cloud travelled about four miles towards the west, along the lower cloud, before it descended to the earth: its being seen to jump over one or two chasms in the cloud, during its passage, was an evidence of its motion in that direction. In the time it was in passing through this space Mr. V. generally counted twenty or thirty, but twice he counted sixty before it struck the earth. During the time of these observations, the small cloud, which supplied such a quantity of the electrical fluid, continued to increase, and particularly in those parts which were towards the wind.

The quantity of electricity supplied by this cloud during an hour, Mr. Varley thinks might be about fifty times as much as it could possibly contain at once; and that it was drawn from the atmosphere along with the vapour which enlarged the cloud, by this last (though positive to the under cloud) being negative to the vapour. From this it seems to follow, that lightning always

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occasions a current of air towards itself; hence a calm preceding a storm, and the fall of the barometer, this being the focus of condensation. He also states, that these phenomena can only be observed when the barometer is low or falling.

As Mr. Varley's remarks on the medical action of electricity are very concise, and not capable of much abridgment, we shall transcribe them.

"First, I think that electricity, entering the body by a good conductor in contact, and going out by another in contact, produces the smallest effect of all, and does but little good or harm.

"Secondly, If it enters the body by a conductor in contact, and has to escape in the manner of perspiration, a strong effect is produced; and this may be confined to one place, and of course be rendered more powerful by the attracting points.

"Thirdly, An effect equally strong, but quite the reverse of the last, will be produced, by placing the patient, insulated, in contact with the negative conductor, and in a damp atmosphere. By this mode of operating the effect takes place all over his body; but if, instead of this, you bring a wet sponge, connected with the earth, near the part affected, it will be confined to that place. In the former case the electricity acts by increasing the natural perspiration; but in the latter, by strongly checking it, or occasioning an absorption of moisture. I think the failures in medical electricity have been where this last application was wanted; for electricity alone, applied in any way, has a tendency to dissipate moisture, which effect the last method enables you to reverse."

2. Since the period at which Mr. Varley made the preceding observations, he states that he had been nearly under a similar storm, and remarked two additional effects which appear to throw some light on the subject. Toward the close of a fine day the air became loaded with vapour, which condensed into larger masses, but exhibited no visible form of clouds. Faint flashes of lightning commenced about these masses, and in a short time the whole was converted into a general mass of clouds, and the sky appeared clear in different parts. A good deal of lightning took place between the upper and lower clouds, attended with a little thunder at intervals: this Mr. V. thinks is clearly accounted for by "the condensation of a very rare cloud into one more dense, but yet quite elastic." But when the spark passes from the lower cloud to the earth, the condensation is into rain, and the atmosphere collapsing upon its nonelastic particles, produces a sharp clap. During the day the wind had been south-west, but when the storm commenced a calm took place on the north-east side of it; and after every large flash of lightning a current of air was felt to proceed westward towards the focus of condensation.

3. At the commencement of Mr. Staveley's observations, the whole surface of the heavens appeared to be covered with one unbroken mass of black clouds, in which the vivid flashes of light-

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ning that rapidly succeeded each other showed no break. Below this apparent black surface a number of white fleecy clouds were visible, which seemed to be luminous throughout, and to resemble summer clouds in the presence of the sun. Looking more steadfastly at these white clouds, Mr. S. perceived a number of bright specks, which appeared like stars seen through a misty cloud. Some of these bright spots gradually increased for some time, and then as gradually diminished until they disappeared. One of them in particular attained the apparent magnitude and brilliancy of Venus in a clear night; but Mr. S. was unable to discern any *body* of light. This meteor was observed to move with considerable rapidity round the edge of the luminous cloud in which it was seen, and then to become stationary again before it died away: its motion continued some minutes. Another meteor, but of a less size, and at a greater distance, was also observed to have a similar motion. During the space of half an hour similar clouds were full of light specks of this kind, and their constant motion produced an appearance resembling that which is exhibited by the galaxy, or milky-way, in a clear night. When both the light and dark clouds had passed away, no stars were visible. No lightning broke from the luminous clouds, but they emitted much light of a pale phosphoric colour; and this appeared to be the nature of that light which formed the body of the meteors. These clouds were at a considerable distance below the upper stratum, and at no great elevation in the atmosphere.

Observations.—It would be difficult, perhaps impossible, to fix upon any one natural phenomenon, which has so much engaged the attention of mankind in general, as that of thunder. The savage, the citizen, and the philosopher, have each listened to this awful voice of Nature under visible manifestations of dread, anxiety, and curiosity. Though the grand and impressive phenomena of thunder and lightning have occupied the minds of philosophers in all ages, no satisfactory explanation of them has been given till modern times, or the more general law of nature developed, of which these are particular instances. Inflammable vapours were long resorted to by way of explanation; and the discovery of gunpowder and inflammable air seemed to strengthen the hypothesis, that extensive strata of these vapours existed in the upper regions of the atmosphere, which being set on fire by any means, would burn away, in an extremely rapid succession, like a train of gunpowder, till the whole in one place was consumed. This, however, could not stand the investigation of the true naturalist, which soon convinced him, that, instead of the effects which he had so frequently witnessed, vast explosions, succeeded by such a motion of the air as would have overthrown all terrestrial objects, would have been the consequence of such inflammation; and that the partial ravages of thunder and light-

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ning bear no resemblance to explosions of elastic fluids. But when the nature of electricity became better understood, and the effects of the charged phial were more attentively observed, it was readily perceived that there existed some similarity between this and the phenomena of thunder and lightning: it was not, however, till towards the middle of the last century that this was viewed in its proper light by the celebrated Dr. Franklin. A dissertation on this subject, written by him, was published in the year 1750, in which he pointed out the resemblance which subsisted between electricity and lightning. He was not satisfied with merely observing this similarity, but soon proved that they were really the same; and actually contrived the means of bringing this tremendous operation of nature, not only within the pale of science, but even within the management of human power, by the construction of what is termed the *thunder-rod*. In was, doubtless, his discovery of positive and negative electricity, and his acquaintance with the curious property of sharp metallic points, which suggested the idea of the whole, and inspired this philosopher with courage to present one of these to the storm, and thus, as it were, to handle the thunderbolt of Jove. This he effected by the assistance of a paper-kite, and which, from being the plaything of his early years, became the means of procuring him immortal fame, and conferring an important service on society.

As the scene of these phenomena is the atmosphere, the next inquiry would naturally be, whence does it derive its electricity? Some of these sources we have already stated at page 294 of our third volume; and we may observe here, that all those operations which excite electricity in other bodies, have the same effect upon air; that electricity is produced by a great variety of common chemical operations, and it is not altogether improbable that any substance, in passing from a fluid to a solid state, and vice versa, is electrical. This, however, appears to us to be a subject which has not yet received its due share of investigation. The conversion of water into elastic steam by great heat is also a copious source of atmospheric electricity.

The circumstance mentioned by Mr. Varley, of the small cloud discharging such a quantity of electricity more than it was capable of containing at once, we conceive may be explained by considering the stratum of clear air as the charged electric; positive on one side, and negative on the other, and coated with clouds which constituted the conducting medium. In this case, as these strata may be some hundreds of yards in thickness, when the first discharge is made, the equilibrium will by no means be complete, as the discharge can effect little more than the superficial parts of the stratum. The remaining electricity of the stratum will then be gradually diffused, until it come within the action of the coating clouds, whence another discharge may take place, occasioning a second clap of thunder, and so on in succession. It appears,

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from Mr. V.'s observation of the lightning travelling in a horizontal direction, that there was an actual transmission of electricity from one end of this line to the other; and the time occupied in the passage might perhaps be caused by the cloud having been a bad conductor. This transmission, however, we do not conceive to be always the case, as shafts of horizontal lightning, extending through a space of several miles, have frequently been observed at the same instant. Is not this, therefore, rather a simultaneous range of discharges than a transmission? This last supposition, we think, is greatly strengthened by the noise of thunder; which is not a snap, incomparably louder than the snap heard in electrical experiments, but a long and very *unequable* noise. This is not at all inconsistent with the series of percussions which cause the sound, having been simultaneous: for as sound occupies time in arriving at the ear, they must be heard in a succession corresponding to the distances of those different points of space where they originate. The unequable and rumbling noise of thunder, no doubt, arises from the different parts of the snap having been differently loud. The well-known and universally acknowledged similarity between the phenomena of thunder and lightning, and those of electricity, also confirms the supposition of a nearly simultaneous series of snaps: for the apparently fine pencil of light which flows from a point positively electrified, when viewed through a glass of high magnifying powers, is observed to consist not of luminous lines, but of lines composed of a series of luminous points. These too are of different degrees of brightness, and even of different colours; both of which are constantly changing. Nor are these lines straight lines, but curves, diverging from each other, and convex to the axis. May not the same curved appearance be observed in what is termed forked lightning? The different degrees of intensity between this kind of lightning and the electric spark can form no objection; for as the great discoverer of this similarity has justly observed, if a conductor, which is only five feet in length and one foot in thickness, will afford snaps of sufficient intensity to agitate the whole human frame, what may we not expect from a surface of 10,000 square acres of electrified clouds? How loud their explosion! how terrible their effects!

On the Causes which have operated in the Production of Valleys. By JOHN CARR, *Esq. of Manchester. No. III.—Phil. Mag. No.* 137.

IN this communication Mr. Carr first enumerates the leading facts and principles which he has presented in his former papers, in order to show that "we have only to appreciate, justly, the incessant, varied, and powerful action of water, in order to ob-

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tain the most rational, simple, and natural elucidation of all the general and principal phenomena in geology." He then examines Mr. Farey's objections to what he has advanced on the subject of general geology, and for the most part shows them to be either unfounded or unimportant; but for both the method and arguments by which this is effected, we must refer such of our readers as are interested in the matter in question to the paper itself, as well as to the earlier parts of this volume for both the controversy, and our remarks on the subject.

On Broom Flax. By Mr. James Hall.—*Phil. Mag. No.* 139.

Mr. Hall states, that in order to procure this kind of flax it is only necessary to steep the twigs of a former year's growth, for two or three weeks, in stagnant water, varying the time according to the heat of the season, or to boil them for an hour in water. The flax will then separate freely from the twigs; and, where there is no machinery for performing the operation, it may easily be peeled or stripped off, in the same manner as hemp is peeled from the stalks. And what still confers additional value on this circumstance is, that when the twigs are cleared of the flax, and steeped some time in boiling water, they become tough and beautifully white, and are worth from one shilling to one shilling and sixpence per pound, for making carpet brooms, and other similar uses. After being stripped from the wood, the flax is only to be well washed in cold water, and dried before it is fit to send to the paper manufacturers, or used for other purposes. He also adds, that Professor Davy had bleached some of it, and that he had seen it spun. Mr. Hall likewise observes, that the fibres of all kinds of mallows are very beautiful, particularly of the *malva sylvestris*, which resembles camel's hair.

Observations.—Discoveries, we conceive, should be valued not only by the benefits which may result from them, but also in relation to that class of society by whom these benefits are most likely to be enjoyed; and we are far from thinking that those are the least valuable which will chiefly affect those members of society which appear to have the fewest comforts of life within their reach. Things of this nature ought to be made as public as possible, and we are always glad when it falls to our lot to participate in their promulgation. Mr. Hall is undoubtedly entitled to the thanks of the community for the way in which he has acted on this occasion. He has sent specimens of the flax to the Royal Society, the Board of Agriculture, the Society of Arts, the British Museum, &c. in order that the discovery might be submitted to more extensive experiments, and its value more justly appreciated, rather than preserve it as a source of private emolument.

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On a Plan for erecting telegraphic Fire-beacons. By Mr. ELMES. —*Monthly Mag. No.* 190.

AFTER some remarks on the numerous conflagrations which take place in the metropolis, and the importance of Insurance Companies, Mr. Elmes observes: "The plan here proposed is, that a sufficient number of fire stations or beacons should be erected, of an appropriate plan and elevation; the design, if elegance were consulted, might be a Grecian or Doric column, raised on a substructure of a cross-like form, that contained stables, engine-houses, and dwelling-room; the abacus surmounted with an iron gallery, ascended by a circular stair-case, and the signal machinery hereafter described. These stations should be within the range of a night telescope, and embrace in their circle the whole of the metropolis, and most populous suburbs. Two or three men should constantly be on duty to relieve each other; one on the abacus of the column, on the look-out, as at His Majesty's telegraphs." Mr. Elmes then describes the machine which he conceives would be proper for making the signals, and observes that it is not of his own invention. The fireman on duty at each station, should be furnished with a map of the metropolis, divided into as many portions as there are distinct signals, and of these this machine is capable of exhibiting twenty-one. The fire-patroles, on discovery of a fire, should immediately give information to the nearest station; the telegraphic machine should then be set to the number in the signal-book corresponding to the division in which the fire was. A rendezvous should also be appointed in the most public street of each division, where the engine-drivers from the other stations might be directed to drive to, and thence be directed to the particular spot where the fire had taken place.

Observations.—The vast losses sustained both by individuals and public companies in this metropolis, and the constant state of alarm which a great number of its inhabitants experience by reason of frequent conflagrations, induce us to regard any plan which has a tendency either to lessen the one or remove any part of the other of these calamities, as justly entitled to the consideration of the public, if not to their adoption. The improvement of the fire-engine has for a long period of time engaged the minds of many ingenious and persevering individuals, and it has now attained a degree of perfection beyond which very little that is really useful can reasonably be expected. The boldness, activity, and exertions of the firemen have, in numerous instances, been such as to merit the approbation of the public; they have done all that men could do in similar circumstances. It is also an ob-

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vious and well-attested fact, that, independent of local circumstances, those conflagrations have generally committed the greatest ravages, in which the times between the commencement of the fire and the arrival of the proper assistance for extinguishing it have been longest: hence it appears that the greatest improvement upon the present system must consist in some means of procuring the requisite assistance at an earlier period, and arresting the flames at the beginning of their progress. We conceive that it does not require much intellectual penetration to discover that if this plan of Mr. Elmes were put into execution, and properly conducted, this desirable end would in a great measure be attained; as it is evident that, in many instances, the engines might be at the place and ready for operation by the time they receive information of being wanted according to the present mode. We hesitate not, therefore, to press this subject, with its attendant consequences, upon the serious attention of the numerous Insurance Companies in this extensive and populous capital.

Mr. E. thinks that a number of buildings of the kind he has suggested, would add much to the ornament of the metropolis. The machine which he has fixed upon for conveying the information by means of signals, is that invented by M. Chappe, in 1793, and adopted in the first chain of telegraphs erected in France. See page 337 of our present volume.

On the Use of Iron for Stairs, and instead of the Timbers of Houses, as a Security against Fire. By Mr. Benjamin Cook.—*Phil. Journ. No.* 107.

Mr. Cook thinks that a security against fire should be provided within doors, rather than depend upon precarious assistance from without; and for this end he recommends the adoption of iron stairs and stair-cases. After noticing some of the chief obstacles to the introduction of such a remedy, he observes, with respect to their construction, "The framing for the stairs, to which the boards are nailed in the present mode, might all be cast, and screwed together. Of course this framing would be considerably lighter in appearance than if made of wood. The front and top of the step, if made of sheet iron, might be attached with six or eight screws to the cast iron framing; and in order to give it a neat finish, a light bevelled moulding might run all round the front of every step, and the jointings be neatly screwed to it with small screws, with heads countersunk into the mouldings." Mr. C. also observes that another, and perhaps a cheaper way, would be to cast the framings with sunk edges, and the top and front of the step in one plate to fit them; and thus the whole might be put together with very little trouble. Some remarks

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then succeed relative to the general appearance and ornamental part of the work. Mr. C. thinks that stairs of this nature and construction would not exceed half the cost of those of stone, with the advantage of having the railing cast of the same material, and to correspond with the rest of the stairs. Common iron staircases he states would be as cheap as those of oak, and, if a manufacture were established, perhaps as cheap as if constructed of any kind of wood.

Mr. Cook then offers some remarks on the advantages which would be derived from the use of iron cast hollow "for joists, rafters, and beams," and other parts of the timber used in buildings.

Observations.—In a former paper, which we noticed at page 170 of this volume, Mr. Cook proposed the substitution of iron for the more expensive kinds of wood, in the construction of various articles of furniture; and he now recommends the same substitute for those kinds which are of a less expensive nature. We hope, however, that Mr. C. has not brought forward this subject as being new, and we would regard the *utility* rather than the novelty of the idea as being his motive; as we had already mentioned this use of iron for "beams, rafters, joists," &c. with the circumstance of their being cast hollow, in our remarks on Mr. C.'s former paper. This we did without conceiving the idea to be new, and we have since been informed that it has actually been put in practice in some parts of this country. Iron stairs, we think with Mr. C. would certainly prove a much greater security against accidents in case of fire, than those which are made of wood, and are therefore, in numerous instances, worthy of adoption, especially in this metropolis and all other great towns, where fires most frequently happen. Hence, we feel it to be our duty to recommend this subject to the serious attention of all who are concerned in either the erection of new houses, or the improvement of those already built; and even should the afflicting circumstance of a conflagration never take place, we are fully persuaded that an ample equivalent to the expense would be obtained in that freedom from apprehension and fear to which the idea of such a security against its dreadful effects, if it were to happen, would give rise in the minds of those who possess it.

A curious Property of single Repetends. By W. Saint, *Esq.*—*Phil. Journ. No.* 107.

Mr. Saint's object in this paper is to prove that any repetend formed by the recurrence of the same digit, as 11111, &c. or 77777, &c. is divisible by any odd number, except 5 and its multiples. And for this purpose he states and demonstrates the following proposition:

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"Every odd number, except 5 and its multiples, is a divisor of a repetend of any of the nine digits; and the number of digits necessary to form the dividend will never exceed the number expressed by the divisor."

With respect to the demonstration of this proposition, when it has been proved to be true for a repetend of units, it may easily be inferred for a repetend of any other digit, as this last is only a multiple of the first, and also the truth of the second part of the proposition is readily derived from the proof of the first part. This is the process which Mr. S. has followed in his demonstration; to which we must refer, for the particular steps, such of our readers as may feel interested in the subject.

Observations.—Though this proposition cannot be ranked among those which are valuable, on account of their usefulness, and ready application to the transactions of real life; yet it may be classed with those that are denominated curious. And Mr. Saint's demonstration will generally be deemed satisfactory, though we think it somewhat more tedious and obscure than was necessary, and this is partly owing to its want of logical arrangement.

On aërial Navigation. By Sir GEORGE CALEY, *Bart.—Phil. Journ. No.* 108.

THE author of this essay thinks that nothing more is necessary for the purpose of bringing the principles of this art into common practice "than the endeavours of skilful artificers, who may vary the means of execution, till those most convenient are attained;" and his opinion has been greatly confirmed by the success of the ingenious Mr. Degen of Vienna, who raised himself into the air by mechanical means. The scheme of flying by means of artificial wings fixed to the arms of a man has been justly ridiculed ever since the days of Bishop Wilkins; but "there is no proof that, weight for weight, a man is comparatively weaker than a bird; it is therefore probable, if he can be made to exert his whole strength advantageously upon a light surface similarly proportioned to his weight as that of the wing to the bird, that he would fly like the bird, and the ascent of Mr. Degen is a sufficient proof of the truth of this statement." Hence, to produce the effect of aërial navigation to a considerable extent, Sir G. C. conceives that "it is only necessary to have a first mover, which will generate more power in a given time, in proportion to its weight, than the animal system of muscles," and he thinks this might be effected by the expansive steam-engine. He states, that by exchanging the boiler in the present construction for a mass of slight metallic tubes, an engine of this kind, capable of doing the labour of six men, might be so made that its whole weight with

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that of its charge for an hour, would but very little exceed the weight of one man. Of this engine he gives an estimate, by way of approximation, in which the whole weight is stated at 163 lbs. He then suggests the probability of using the expansion of air, with the advantage of lessening the weight of the apparatus, by the sudden combustion of inflammable powders or fluids, in the manner lately shown by some of the French chemists. He likewise mentions an engine which was made some years since in this country, and worked by means of inflamed spirit of tar. The power of this engine was equal to that of one horse, and its weight did not exceed 50 lbs. He also adds, that "probably a much cheaper engine of this sort might be produced by a gas-light apparatus, and by firing the inflammable air generated, with a due portion of common air, under a piston."

Having taken this general view of his subject, and brought the accomplishment of it within the range of probability, Sir George proceeds to explain the principles of the art. For this purpose he analyzes the simple action of the wing in birds; and illustrates this analysis with figures, to which we must refer. After this he observes, that "the whole problem is confined within these limits, viz. to make a surface support a given weight by the application of power to the resistance of air." Experiments on the resistance of air are next considered; and this writer concludes, from some extensive ones of his own, that a square foot of surface sustains a resistance of one pound, when moving perpendicularly through the air, with a velocity of about 23.6 feet per second. The experiments of the French Academy proved that the resistance of a plane moving through the air with an inclination of six degrees to the horizon, was $\frac{2}{7}$ of the resistance which the same plane met with when it moved perpendicularly; hence a square, when carried forward with this inclination, would sustain $\frac{2}{7}$ of a pound; and, admitting the resistance to vary as the square of the velocity, it would receive a pressure of one pound when the velocity was increased to 27.8 feet per second. From the whole of the preceding remarks and experiments the author concludes, that he may safely state, "that every foot of such curved surface, as will be used in aërial navigation, will receive a resistance of one pound, perpendicular to itself, when carried through the air in an angle of six degrees with the line of its path, at a velocity of about 34 or 35 feet per second."

The writer's next object is to apply what he has advanced to the theory of aërial navigation; and this he does by the assistance of a figure: but the following description will convey a just idea of his method. He supposes a surface or sail to be made of thin cloth, of a firm texture, containing 200 square feet; and that the weight of the man and the apparatus is 200 pounds. Then, if a wind blow with the velocity of 35 feet per second, in a certain direction, at the same time that a cord in that direction sustains a

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tension of 21 lbs. from being fixed to the machine, the whole apparatus will be suspended in the air. But it is perfectly indifferent whether the wind blow against the plane, or the plane be propelled by any means against the air with an unequal velocity. Hence, if this machine were drawn forward by the cord under a tension of 21 lbs. and with a velocity of 35 feet per second, the whole would be suspended in a horizontal path. Now, if, instead of this cord, any other propelling power were generated in the same direction, and with the same intensity, the same effect would be produced, and aërial navigation accomplished. As the angle of inclination between the direction of the wind and the plane decreases, the requisite propelling power also diminishes.

The author then promises further communications on the subject, and observes, "that perfect steadiness, safety, and steerage, I have long since accomplished, upon a considerable scale of magnitude." He next describes a simple machine, which will ascend into the air by mechanical means; and concludes with stating what he conceives to have been the method adopted by Mr. Degen, when he ascended at Vienna.

Observations.—Both the novelty and importance of this subject, we are persuaded, will induce most of our readers, and particularly such as have not seen the original, to examine this paper with that degree of eager curiosity which we have experienced; and only a small portion of attention will be necessary to convince them, that the moment which shall realize the subject will constitute a new epoch in the affairs of men. How far we ought to agree with Sir George Caley's opinion, as expressed in the following sentence, we cannot decide, as we are not yet in possession of all the facts and observations upon which it is founded. He says, "I feel perfectly confident that this noble art will soon be brought home to man's general convenience, and that we shall be able to transport ourselves and families, and their goods and chattels, more securely by air than by water, and with a velocity of from 20 to 100 miles per hour." In this state of the case, therefore, any further observations of ours would be premature, and might, perhaps, rather tend to retard than to facilitate the desired end.

Experiments on the Production of Sounds in Vapours. By M. Biot. —*Mémoires de la Société d'Arcueil.*

After some remarks on the mediums in which philosophers have made their experiments respecting the production and propagation of sound, M. Biot observes, that none have been made in vapours, at least with which he is acquainted, though a research of this kind is well calculated to excite curiosity. He also ob-

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serves, that the quantity of vapour formed in a vacuum depends solely on the dimensions of the space and the temperature. If this vapour be slowly compressed into a less space, the elastic force will not be increased, as in a permanent gas, but part of the vapour will assume a liquid state. On the contrary, when the space is enlarged, a fresh quantity of vapour arises to fill it, without affecting the elasticity of that by which it is already occupied.

Now, if the vibrations of a sonorous body take place in such a medium, each of these will diminish it on the one side, and increase it on the other, by a small quantity of vapour passing into a liquid state, and the contrary. These condensations and expansions will only take place in the immediate vicinity of the vibrations, without being extended into the surrounding mass of vapour; and consequently the sound cannot be transmitted. But if it be supposed that the sonorous body, by compressing the vapour by its quick vibrations, mechanically disengages from it a quantity of caloric, the effects of these vibrations will no longer be the same. The condensation and expansion will be prevented by the liberated caloric; and the phenomena thus produced will consist of increments and decrements of elasticity successively and momentarily produced. Their effects will extend through the whole mass, and the production and propagation of sound will be the consequence. M. Biot, therefore, regards experiments on sound produced in vapour as highly proper to decide the question, whether or not heat is really disengaged by the vibrations of sonorous bodies in an aëriform medium? For if any sound be produced, it can only be considered as the sole effect of the caloric set free by the vibrations of the body.

It was with this motive that M. Biot instituted these experiments; and which he afterwards repeated at Arcueil, in the presence of the two Berthollets and Laplace, and who were fully convinced of the truth of the following results. The apparatus he used for the purpose consisted of a glass balloon, capable of containing about 36 litres; its orifice was closed by a perfectly tight stop-cock, and it was so constructed that any portion of liquid might be introduced without admitting the external air. The sonorous body was a small bell, fixed by a fine string to the stop-cock.

A vacuum was first formed, with the greatest care, in the interior of the apparatus, which was then held by the stop-cock, and the bell put in motion, but no sound was heard; thus confirming all previous experiments of the same kind. A small quantity of water was then introduced, part of which was changed into vapour, and the sound immediately became perceptible. The density of this vapour was very small, the temperature not exceeding 19 degrees of the centesimal thermometer. In order to increase the density of the vapour more water was added, and the temperature was raised to about 46 degrees, when the sound became very sen-

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sible, and was heard at a distance from the apparatus: there yet remained a part of the water in its liquid state. When the temperature of the machine was lowered, and a considerable part of the vapour produced by the additional heat precipitated, the sound was sensibly weakened.

An equal quantity of alcohol was next introduced to the water. The specific gravity of the alcohol, considering that of water as 1, was 0.823; consequently the vapour formed from this mixture possessed both greater elasticity and density than that from water at the same temperature, and the sound was likewise much more sensible: hence it follows, that sound is also propagated in the vapour of alcohol.

The atmospheric air was next permitted to enter the balloon, until the equilibrium was restored, when the sound of the bell was heard at the distance of 145 metres; the temperature being 17.75, and the external pressure of the atmosphere 0.7613. Ether was then chosen, on account of its great density and elasticity; and the vacuum being again formed, more of it was introduced than could be raised in vapour at that temperature. The specific gravity of this ether was 0.759; the elastic force of its vapour, measured by the barometer, 0m. 35.49; and the temperature 17°.56. The sound was sensible at the distance of 131.5 metres. The results of all these experiments show, that sound is produced and propagated in vapour as well as in permanent gas; and the cause of this is evidently the variations of temperature occasioned by the vibrations.

Observations.—It may, perhaps, be necessary, for the information of some of our readers, to observe, that the *Société d'Arcueil* derives its name from the country-seat of the elder Berthollet, near Paris, where the members meet once a fortnight, to spend the day in philosophical occupations. The members of this society are the elder Berthollet, Laplace, the younger Berthollet, Biot, Gay-Lussac, Humbolt, Thenard, Decandolle, and Collet-Descostils. At their meetings the latest scientific journals are consulted; philosophical papers are read, and their merits discussed; new experiments are suggested, and others repeated, as appears in the present instance. This is an institution, so well calculated for the advancement of science, and so much activity and perseverance have been evinced by its members, that we should be extremely glad to see similar institutions established in this country. The errors that might be avoided, the mistakes corrected, and the new and important lights thrown upon abstruse and difficult subjects by the united efforts of a few ingenious and learned men, could not fail of imparting to their individual exertions a value which they could not otherwise attain, and of ultimately contributing to the real advancement of science.

It was by supposing that caloric was disengaged by the vibra-

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tions of sonorous bodies in an aëriform medium, that Laplace was able to reconcile the mathematical theory of the propagation of sound in air, with the results of experiments; and the correctness of this supposition appears to have been fully proved by this series of experiments. Hence the necessity of regarding this cause in the mathematical theory of the propagation of sound is evident. Nor can the circumstance of this not being indicated by the thermometer be urged as any objection, as that is an instrument which cannot be affected by small, successive, and momentary changes of temperature. The actual developement of caloric being thus established, it only remains to determine its quantity, and the consequent increase of elasticity dependent upon it, in order to solve the difficult problem relating to the difference between the result of the mathematical propagation of sound and that of observation, in a direct manner: for as the general expression for the space through which sound passes in an expansible fluid is $t=\sqrt{\frac{2ge}{d}}$; where g represents the space through which a heavy body would fall in the time answering to that required for the sound to pass through the space s, and e and d, the elasticity and density of the expansible fluid; it is evident that when e shall be accurately determined by experiment, the whole expression will become known.

We see no reason to doubt that heat is evolved whenever vapours are compressed; a modern author has even attempted to calculate the quantity of heat which is probably extricated during the condensation of steam (see Young's Philosophy, ii. 409). But we cannot admit that M. Biot's reasoning on the subject is conclusive: it is not very probable that a space should have become so perfectly filled and saturated with vapour, that the least imaginable compression should produce a condensation; and it must be remembered, that the compression concerned in sound is always extremely minute; and even if such a condensation and deposition did take place in the course of the first vibration, the fluid would not have time to evaporate again before the occurrence of a second, and the vapour would remain in a state capable of admitting the given compression without any further deposition.

Description of a new Anemometer. By Richard Kirwan, Esq. *LL.D. P.R.I.A, F.R.S. &c.—Trans. R. Irish Academy*, 1808.

This apparatus consists of a pole fixed in a vertical position, above a building, through the roof and ceiling of which it passes into an apartment below. The bottom of this vertical axis or pole is sheathed with a steel point and socket resting on a stand or

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frame, so as to turn with ease, and avoid friction as much as possible. Across the top of this axis a square tube is fixed, containing a sliding rod which moves freely over brass rollers, and has a light wooden frame one foot square, covered with very thin sheet brass, and strongly varnished with copal, fixed to one of its extremities. This is the surface upon which the wind acts in moving the machine. Towards the bottom of the pole there is fixed a frame of light wood, in which several weights are confined, one above another, in grooves, in such a manner as to work up and down with the greatest facility. These weights are connected together by cords, and marked 1, 2, 3, 4, &c.; the space between each of these, when drawn up by the force of the wind, is about one inch, and each weighs one pound avoirdupois. A line is fastened to the top, and passes along the pole to the transverse tube, where it passes over a brass pulley fixed under the sliding rod above mentioned, to which it is fastened between the pulley and the extremity bearing the square surface presented to the wind. This line is composed of a number of common sewing threads, laid in different directions, well waxed, and enclosed in a cotton case, to prevent as much as possible any variation in its length by changes of the atmosphere. A scale and index is attached to the frame in which the weights are confined, for the purpose of marking the greatest force of the wind during the absence of the observer. This index is connected, by means of a string passing over a pulley in the upper part of the frame, with a small weight by which it is counterpoised, and which rests upon a hand fastened to the weight No. 1. By this contrivance the small weight is caused to rise, and the index to descend, as the force of the wind increases; and the latter remains at the number of pounds raised by the wind, though they should fall down into their proper places on the wind's abating. A vane or weathercock is placed on the top of the pole, to show the direction of the lighter winds, which could not be known by the anemometer, on account of the weight of its necessary appendages. This machine may also be rendered more complete, and made to answer the purpose of an anemoscope, by applying an index to that part of the pole or axis which enters the apartment, and having the points of the compass described on any plane surface attached to the ceiling.

Observations.—The first instrument of this kind, of which we have seen any account, was invented by Wolfius, in the year 1708, and the description of it first published in his *Areometry* in the following year. Descriptions may also be seen in other parts of this author's works. This machine consisted of sails, like those of a windmill. When these were turned by the force of the wind, they raised an arm loaded with a weight to a certain elevation, the angle of which was pointed out by an index prefixed to the instrument. An account of another instrument of this kind is

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given in the Memoirs of the Academy of Sciences for 1734, said to have been invented by M. D'Ons-en-Bray, and to be capable of expressing on paper not only the several winds that had blown during the space of twenty-four hours, and at what hour each began and ended, but also the different strength or velocity of each. Another anemometer is described in the Philosophical Transactions, in which the wind blowing against a flat board, causes it to move along the graduated arch of a quadrant; and the number of degrees it advances shows the comparative force of the wind. In the same work, for 1775, Dr. Lind gives a description of a very ingenious portable *wind-gauge*, by which the force of the wind is easily measured. This consists of a bent glass tube, the two legs of which are parallel to each other, about 8 or 9 inches in length, and $\frac{4}{10}$ths of an inch in diameter. The diameter of the bent or connecting part is only about $\frac{1}{10}$th of an inch. On one end of this tube another of metal is fixed, having its mouth directed horizontally to receive the wind. The whole is fixed on a stand, and capable of being turned in any direction. When it is used, a quantity of water is poured into the tube, and the force of the wind entering the horizontal orifice, causes the water to ascend in the other leg, and the quantity of its ascent, which marks the force of the wind, is determined by a scale fixed to the side of the tube. This instrument of Dr. Lind shows only the force or momentum of the wind, and not its velocity. This latter, however, may be obtained from the former by means of some experiments made by Dr. Hutton, at the Royal Military Academy, in the years 1786, 1787, and 1788. From these experiments, it appears that a plane surface of a foot square suffers a resistance of twelve ounces from the wind when blowing with a velocity of 20 feet per second; and that the force is nearly as the square of the velocity. Hence, from knowing the one, the other may be obtained by proportion. In one instance Dr. Lind found the force of the wind to be equal to $34\frac{2}{10}$ pounds on a square foot, which answers to a velocity of 93 miles an hour; and which, since M. Garnerin's celebrated aërial voyage to Colchester on the 30th of June 1802, must be reckoned within the bounds of probability. Mr. Leutmann improved upon Wolfius's anemometer, by placing the sails horizontally instead of vertically. Mr. Benjamin Martin also improved upon this last, by making the axis of the sails conical instead of cylindrical. This axis resembles the fusee of a watch, having a cord with a weight attached to it, winding upon it from the smaller to the larger end, as the force of the wind increased. Mr. Bouguer likewise contrived a very simple apparatus, by which the force of the wind on any given surface may readily be ascertained. This is a hollow tube, in which is fixed a spiral spring, that may be more or less compressed by a rod, passing through a hole in the tube. Having observed and marked the different points to which the spring is capable of being

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depressed by given weights, the force of the wind on the given surface will be indicated by the point of the spring.

The present contrivance we conceive to be well adapted for obtaining an accurate measure of the force and velocity of wind, so desirable to meteorologists. We, however, think it might be improved by fixing the cord which is connected with the weights near the contrary end of the slide to that to which it is at present fixed, as that would cause the slide to be forced inwards instead of outwards by the wind, and remove part of the apparatus from between the wind and the surface upon which it acts. The circumstance of the weights being so heavy as one pound each appears to limit the operation of this machine too much; for as the force is indicated by the number of weights raised, it can only be known for *exact* pounds, while that answering to the intermediate parts is not ascertainable, according to the present description of the apparatus.

Report of a Memoir of Mr. Hassenfratz, respecting the Alterations which the Light of the Sun undergoes in traversing the Atmosphere. By M. Hauy.—*Journ. de Physique, Vol. LXVI.*

As the light of the sun is composed of an infinite number of tints of different colour, the union of which produces white, it would always appear so to us, if its rays were not affected in passing through the atmosphere. But circumstances frequently occur, in which we see it under a variety of colours; sometimes with its natural whiteness, at other times it appears yellow, orange-coloured, or red. Mr. Hassenfratz imagines these different appearances to depend upon the state of the atmosphere, the difference of latitude, and the elevation above the level of the sea. The ultimate cause he supposes to be, what Newton had before observed with respect to transparent mediums, the suppression of a part of the solar rays in their passage through the atmosphere.

The object of this author was, therefore, to ascertain the number and quality of the intercepted rays, and which give rise to the different tints in the appearance of the solar disk. The means he employed for the accomplishment of this end are founded upon a rule laid down by Newton, for determining the colour produced by a given mixture of rays of various kinds, taken from those which constitute the solar spectrum. Hence it follows, that if we know the kinds of rays which the atmosphere intercepts during the passage of the solar light, we may infer the colour produced by the mixture of the remaining species; and consequently judge whether it be the same as that under which the disk of the sun is seen. Here it is observed, that the mixture producing the given colour may be more or less compounded, since the species of the colour does not change by the addition of parts of the spectrum

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at equal distances from the point which constitutes the centre of the colour. Mr. H. also conceived, that observations on the solar spectrum, produced by the refraction of the prism, would enable him to ascertain this point.

Under this idea, on the 13th of January 1807, at ten o'clock in the morning, he remarked that the violet and part of the indigo were wanting. Hence, according to Newton's rule, the remaining colours ought to produce yellow; and the disk of the sun at that time appeared of that colour. On the same day, at noon, the spectrum exhibited all the colours, and the sun appeared white: but at four in the afternoon the violet, with a greater portion of the indigo, had again disappeared, and the sun's disk was of a deeper yellow than in the morning. At a quarter of an hour afterwards the spectrum was further shortened on the same side, and the disk of the sun inclined to an orange colour. This author also remarks, that he has observed varieties in the number and extent of the coloured arcs of the rainbows seen at different hours of the day, which were the effects of this suppression of part of the rays. Mr. Hassenfratz also presented several drawings of the spectrum, taken at the different times of observation, to the mathematical class of the National Institute.

Observations.—Experiments of this kind appear to possess considerable value, as they tend to explain, in a natural and satisfactory manner, some appearances, relative to which we had nothing which could be deemed conclusive. We, however, think, from the account of them given by Mr. Hauy, that they were much too few in number to establish the point with all the certainty that could be wished, and that, therefore, it deserves to be further pursued by philosophers, particularly as it ought to be attended to in all experiments on the decomposition of light. What Mr. H. has remarked respecting the rainbow we can confirm from personal observation.

Extract of a Letter from Professor Picot, of Geneva, to the Editors of the Bibliothèque Britannique, on Comets.—Journ. de Phys. Vol. LXVII.

Mr. Picot commences his remarks with the comet of 1807, which was discovered in September, in the constellation of the Serpent, and travelled for the following months, nearly at the rate of a degree a day through those of Hercules and the Lyre, until it ceased to be visible. Mr. Olbers took all favourable opportunities of observing it till the 19th of February 1808, when his observations were interrupted by illness. Mr. Bessel continued his observations in the Observatory at Lilienthal, near Bremen,

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till the 24th of the same month, and the following elements were calculated by him, from these observations, and others taken at Bremen. The time of its passing the perihelion, in mean time at Paris, was September 18th at 7 ho. 59 min. 48 sec.; the longitude of its perihelion, 9 sin. 0 deg. 56 min.; its perihelion distance, the mean distance of the earth being unity, 0.64648; longitude of the ascending node, 8 sin. 26 deg. 46 min. 3 sec.; and the inclination of its orbit 63 deg. 10 min. 53 sec. Mr. Bessel has computed the period of its return to the perihelion at 1900 years; but Mr. Olbers thinks the accuracy of this computation is not to be depended upon. Mr. Picot observes, it is much to be wished that some astronomer would collect all the accurate observations made at different places, and revise the calculations, in order to arrive at some probable conclusion on this point. He conceives that the predictions relative to the two comets of 1456 and 1532, the former of which has since been seen four times, and the latter twice, demonstrate the general proposition, that these returns may be predicted. He also observes that, if Mr. Bessel's calculation with regard to the return of this comet be correct, the approaches it may make, during so great a period, to the large gravitating bodies of our system may cause perturbations in its course which cannot be calculated.

A table containing the elements of the last 21 comets forms a part of this communication. These have been observed, and several of the elements calculated by Mr. Olbers. From this table it appears that the number of those comets which approached nearer to the sun than the earth's mean distance from it, is double these of which the nearest distance exceeds that of the earth. Four of them approached within less than a tenth of the earth's mean distance, and four others nearly within one fifth of it. With respect to motion, 12 of these have been retrograde, and nine direct. Some remarks are then made on the difference between planets and comets, with respect to the regularity of the one, and the irregularity of the other. All the planetary motions are from west to east; and M. Laplace says, in his *Systéme du Monde*, that the planetary system exhibits to us 42 movements in this direction, and that it is four millions of millions to one that this arrangement was *not* the effect of chance. But as the eccentricities of comets, the inclination of their orbits, and the direction of their motions, are subject to so great a variation, Mr. Picot concludes, that "different final causes must have presided over the different formation and destination of the planets and comets."

Observations.—We have previously noticed two papers on the comet of 1807; that of Mr. Lowe, at page 14, and Dr. Herschel's at page 373 of our 4th volume. Some of the elements given in the present instance, differ considerably from those stated in Mr. Lowe's paper; and as his were computed from some of the early

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observations, we are inclined to give the preference to those of Mr. Bessel as being more correct. Mr. B.'s perihelion distance agrees very well with both Mr. Lowe's and Dr. Herschel's, the former being 0.64802, and the latter 0.647491. The inclination of the orbit, as given by Mr. B. only differs a few minutes from that computed by Mr. Lowe.

What Mr. Picot has observed relative to the perturbations which this planet may receive in its course during a revolution of 1900 years, from its approach to the large gravitating bodies of *our* system, appears not to have been the result of very serious reflection. The question is, if this be the true period of its revolution, is the whole of its orbit comprised within the limits of *our* system? From the circumstances attending its appearance, however, we think that this question ought to be answered in the negative; and if so, is it not more probable that the greatest perturbations may arise from the influence of the bodies of some other system, into which the distant part of its orbit must enter? In this part the velocity of its motion is the least, and the perturbations would be most easily effected.

The distance it must attain, by constantly receding from the centre of the solar system for the space of 950 years, is so immense, and the consequent approach to that of some other system so probable, as strongly to favour the hypothesis of Mr. Lambert and Mr. Cole, mentioned at page 4 of our present volume.

Mr. Picot's conclusion at the termination of his paper, we conceive to be totally unworthy of its author; for "different final causes presiding over, &c." even in its best *sense* is only *nonsense*; and we are, therefore, left to guess at his meaning. If he mean that planets and comets owe their being to different efficient causes, we cannot but regard the idea as an impious one, involving in it a *plurality* of first causes, in opposition to the whole tenor of Revelation, which ascribes all things to the will of *one* Supreme Being, who is "God over all." If his meaning be, that they were formed to answer different ends, and to fulfil different purposes, then we conceive it to be correct; for we are unable to regard them in any other light than as parts of one great, harmonious, and beautiful whole. With respect to the purposes to be accomplished by this difference, we have already ventured to give our opinion in the remarks on the first paper in this volume, viz. the populousness of the universe; and we may now add, that this opinion is more firmly established by every serious contemplation of the subject.

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Report on a Method employed in the Mint (in Paris) to destroy the Smoke of Steam-engines. By Messrs. GUYTON MORVEAU *and* DE PRONY.—*Annales de Chimie.*

IN order to effect this purpose, the boiler of the steam-engine is so constructed, as externally to represent a solid of revolution generated by a trapezium, terminated at the top by a semicircle. The fireplace beneath the boiler is a circle, and the grate a square inscribed in the circle. Opposite to the door at which the fuel is introduced, an opening permits the flame to enter a channel made in the brick-work, for the purpose of circulating round the boiler. After one entire revolution, this channel terminates in a chimney which communicates with the external air. Besides this channel which encompasses the boiler, another proceeds from each side of the door to the opening on the opposite side. The two channels are situated below the other, and make each half a revolution. Upon the door of the fireplace being closed, the two currents of air which enter these channels, unite in one at their further extremities, and this rushes along with the flame into the channel of circulation; by which means the temperature of the vaporized part of the fuel, which would produce the smoke, is maintained at a sufficient height to decompose the air, and is thus consumed by a fresh supply of its oxygen. The caloric thus disengaged by the combustion of the smoke, also serves to facilitate the heating of the boiler. The effects take place during the whole of the time that the door of the apparatus is closed; but when this is opened for the purpose of introducing fresh fuel, the combustion of the smoke immediately ceases. This slight inconvenience, these reporters observe, might be avoided by constructing the furnaces after the manner of those called athanors, into which the fuel is introduced by means of a harry, and consequently the fireplace is kept constantly closed.

Mr. Gengembre, under whose direction this method was put into execution, does not claim the merit of its invention, as he was previously acquainted with the processes employed by Messrs. Clement and Desormes at their manufactory of green vitriol at de la Garre, Paris; and also at their alum-works at Verberie. M. Champy, jun. had likewise made use of a similar process for the same purpose at his gunpowder-works at Essone. A construction for this intent was also formerly known in France. It was invented by Dalesme, and is described in the collections of the Academy of Sciences, prior to its organization in 1669. Messrs Clement, Desormes, and Champy, disclaim any knowledge of the inventions of Messrs. Watt and Robertons, when they first employed themselves on processes of this kind. These reporters state that

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they have not seen a description of Mr. Watt's method in any of the works which give an account of the English inventions that have come to their knowledge; and add, that they should have known nothing of it but from its being prefixed to an engine which was erected by Messrs. Bolton and Watt, in 1790, at Nantes, which yields no smoke.

It is also here stated, that the most important conditions for securing complete combustion are the following: 1. Such a disposition of the fireplace, that a current of air may flow into the flue or chimney, by which the gasses disengaged by the combustion are permitted to escape. 2. In introducing such a quantity of air into contact with the fuel as may furnish, when decomposed by the heat, a sufficient quantity of oxygen for uniting with all the molecules of the fuel which are capable of combining with it.

Observations.—We regard this as a very simple method of relieving the inhabitants in the neighbourhood of steam-engines from that annoyance which is so frequently experienced from their smoke. The authors of this report, however, are not correct respecting Mr. Watt not having published any account of his invention, as may be seen by turning to page 216, volume 4th, of the Repertory of Arts, Old Series. His method consists simply in causing the smoke or flame of the fresh fuel to pass through fuel which had ceased to smoke and is exceedingly hot, in its way to the flue or passage by which it escapes from the fireplace. By this means the grosser parts of the flame or smoke are converted into pure flame, free from smoke. Messrs. Robertons' patent for the same purpose was obtained in 1801, and the specification was published in the 16th volume of the Repertory, First Series.

Remarks on some Points in Hydrography. By M. Leblanc, *Officer in the French Navy.—Journ. de Phys. Vol. LXV.*

This writer observes, that the latitudes and longitudes of the chief ports in the Gulf of Mexico have not been fixed, and, therefore, are not to be found in either the French Connoisance des Temps, or the English Requisite Tables: hence navigators are obliged to have recourse to the most modern charts. In these M. Leblanc thinks there exists an error in the longitude of 52′, with respect to all the ports of the Gulf. At four o'clock, on the 25th of January 1807, the Foudroyant cast anchor at a small distance from the Great Bahama, when Lena Key bore N. 80° E., and that of Azena N. 45° E. The longitude given by the time-keepers was 80° 17′ 30″, and that by the chart 82° 15′; therefore the Gulf is placed too far towards the west by 52′ of a degree. The chart used was the French general chart of the Atlantic, cor-

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rected in 1792. All care was taken to ascertain the correctness of the time-keepers; and when the ship arrived at Brest, after a passage of 35 days, the absolute error was found to be only 7′ of a degree.

The latitude of one of the principal marks in the old Bahama channel, styled Green Key by the French, and Chesterfield by the English, was found to be 22° 7′ instead of 21° 55′, as given in the Connoissance des Temps. Captain Fonsera, of the Portuguese navy, and superintendant of the harbour of San Salvador, one of the chief ports of Brazil, informed this writer, that its latitude was 13°, and longitude 42° 25′; while an English work stated it to be 12° 46′ and 41° 5′. M. L. however found, both by lunar observations and his time-keepers, that the true longitude was about 41° 5′, and that the latitude of Point St. Antony was 12° 59.8′. The variation of the needle at this port in 1806 was 10° 20′ E.

Observations.—The difference between 82° 15′ and 80° 17′ 30″ is not 52′, as stated above, but 1° 57′ 30″. We have given the numbers as in the original, not being able to ascertain where the error was. This correction, if future observations prove it to be accurate, we deem of great importance. The want of good charts of this dangerous part of the ocean, has often been felt and complained of by navigators; and we think that it is much to be regretted that those who visit distant parts, and have it in their power to make correct observations relative to the latitudes and longitudes of the different ports they touch at, have not employed these opportunities for determining this important object. That many errors yet remain to be corrected in our best tables of this kind we have very little doubt; and the error which has lately been discovered in the longitude of Madras is one striking instance. See page 374 of our 4th volume. The Chesterfield, above mentioned, must not be confounded with another place of the same name on the south of the great bank of Bahama, and nearly in the same latitude.

REVIEW OF SPECIFICATIONS OF PATENTS,

PUBLISHED IN THE REPERTORY OF ARTS, DURING THE MONTHS OF OCTOBER, NOVEMBER, AND DECEMBER, 1809.

Mr. CHARLES SEWARD*'s Patent for a new or improved Street-lamp and Burner, and Lantern Head, for Street and other Lamps and Lanterns. Dated July* 1809.—*Repert. of Arts, No.* 89, *Second Series.*

THE head of Mr. Seward's lamp consists of a cover, and a pipe or chimney passing through its centre, and reaching down to within about two inches of the lamp. This pipe or chimney is

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open at the lower extremity, and covered at the upper end by a scalloped cap, which allows the smoke to escape, but excludes the wind and rain. In the rim of the cover several holes are pierced, for admitting a supply of air to enter the globe; and the wind and rain are prevented from entering the globe by the same means, by an inner rim fixed to its upper edge. With respect to the chimney the patentee observes, "that its size is not very material. In general for street-lamps its length may be about ten inches, and its diameter about three inches. The height of the chimney above the lamp ought to be determined by experiment, for it varies with the size of the chimney and the size of the lamp: a chimney of the above dimensions for a street-lamp may be placed about two inches above the top of the lamp."

The body of the lamp or reservoir for the oil is made very shallow, in order that the oil may be always near the flame, and have as little as possible to ascend up the wick. It is also made perfectly close, except where it communicates with a small projecting part which contains the wick, and into which the oil is poured. The wick-tube is flat, and should not be less than one sixth of an inch wide: its length should be such as to touch the bottom of the reservoir, having one side cut away to admit the oil to the wick. This tube is soldered into a cover which fits the projecting part of the lamp, and rises about one eighth of an inch above it. The patentee observes, "The proper wick for these lamps is the broad flat wick, commonly called garter or ribbon wick, which I prepare by soaking it in melted tallow, and afterwards squeezing or pressing out the superfluous grease. Wick thus prepared not only burns with a clearer flame, but lasts longer, as the incineration of the wick and formation of the crust take place slower than if the wick were unprepared." These wicks are to be cut into pieces one quarter of an inch longer than the wick-tube, and should be renewed every night.

Observations.—We think that lamps of this construction will be found preferable to those which are now in general use: that part, however, of these improvements, which relates to the formation of the reservoir, wick-tube, &c. constituted a part of those for which Mr. Seward obtained a former patent, and of which we have given an account at page 61 of this volume. The wicks prepared in the way which Mr. S. describes will doubtless answer better than those that are unprepared. The circumstance of their being cut into such short lengths will necessarily occasion both a waste of wick, and some trouble in changing them so frequently

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Mr. Curr's *Patent for laying a Rope, or, in other Words, of twisting and forming the Strands together that compose the round Rope. Dated November* 1805.—*Rep. of Arts, No.* 89, *Second Series.*

In this method of making ropes it is necessary that there be a wheel or engine for twisting the strands at the upper end of the ropery, and also another moveable one towards the lower end of the same. The large tooth wheel in each of these engines, which drives round the smaller nut wheels, should contain 94 teeth. Three nut wheels, containing fourteen teeth each, are occasionally to work into the large wheel at the top of the ropery; three more, containing 25 teeth each, and also three others with 50 teeth each. These are to be put into gear to twist up the strands, and to accommodate different sizes of ropes. In the same manner there are three small wheels working into the tooth wheel at the bottom of the ropery which have eight, three more which have fourteen, and also three which have twenty-eight teeth each. In order to twist a rope regularly from end to end, Mr. Curr attaches a reel, or other similar apparatus, to the engine at the lower end of the rope-walk, in such a manner as to point out and command the speed of the moveable top generally used in laying a rope. The nut wheels employed at both ends should be proportioned to each other, and the number of turns in the same time be made at each end, to give the rope a regular twist throughout. Mr. C. then enters into a further explanation of these principles, for the purpose of enabling the mechanic to construct and apply them properly. For laying a rope three inches in circumference, he directs that the diameter of the reel, by which the motion of the top is regulated, be about 30 inches; and to make a shroud-laid rope, of either greater or less circumference than this, the diameter of the reel is to be increased or diminished two inches for every three eighths of an inch which the ropes are less or more than this in circumference. Several other directions are also given respecting varying the number of teeth, &c. for the purpose of making the rope more or less hard, as may be required; as well as relative to some other particulars.

Observations.—The purport of this patent is to give an uniformity of twist to the rope throughout its whole extent, by regulating the motion of the top. This regular combination of twists is certainly a very desirable object in the art of ropemaking; but we apprehend that in the present instance Mr. Curr has proceeded upon a false principle for the attainment of this end. He observes, "that it shall require an equal number of turns on the wheel, at both ends of the ropery, to give the proper twist or hardness to

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the rope, so that, the workmen making one revolution or turn of the one wheel or engine at the same time as the workmen do on the other end, a regular proportion of twist is effected and kept up." Mr. Curr's investigation and experience, however, have probably taught him by this time, that, in order to effect this purpose, the number of twists put in at the strands, and that taken out by the rope during the process of laying, should not be the same; or, in other words, that the wheel at each end should not make the same number of revolutions during the process. The necessary turns of the strand are fewer than those of the rope; the proportion of these turns to each other depends upon the obliquity of the spiral of the rope, and increases or decreases with it; so that, when the obliquity of the twist is determined, this proportion also becomes known. There is, therefore, reason to doubt whether this mode of Mr. C.'s will answer the purpose any better than that which has long been in general use among rope-makers, and which appears very simple, and, under proper management, capable of a considerable degree of accuracy. This method is to mark the strand at some distance from the strand-hooks at the upper end of the ropery, and where they cross some of the stake-heads. The motion of the top is then given by the twisting of the rope by means of the engine at the bottom of the ropery, and the velocity of this motion is regulated by the foreman according to the spiral which the strands are intended to form round their common axis. In this process, if the marked parts of the strands be observed to advance towards the top, it indicates that less twist has been put into the strands than has been taken out by the rope, and the men at the upper wheel are directed to heave faster: but, when the marks recede from the top, the contrary is the case, and the men are directed to heave slower. For further particulars on this subject, see the different volumes of our work, and Mr. Chapman's "Treatise on the progressive Endeavours to improve the Manufacture and Duration of Cordage."

Mr. SAMUEL CLEGG's *Patent for a rotative Engine, the Piston of which makes a complete Revolution at a Distance from the revolving Axis, Shaft, or Cylinder. With Observations by the Patentee. Dated July* 1809.—*Rep. of Arts, No.* 90, *Second Series.*

IN this apparatus of Mr. Clegg, one of the principal parts is a large circular piece of cast iron, or any other material which may be found to answer the required purpose. In the under side of this are cut two concentric circular grooves, of a convenient width, the outer one being the deeper. The remaining part of this surface, except a circular part near the outer edge, is cut down to any given depth less than that of the inner groove. Into the

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deeper of the two grooves, a convenient number of pieces, which the patentee terms segments of a circle, are fitted, which occupy the whole groove, except a small space allotted to the adjusting screws or springs, which keep these pieces close together. These segments are of a breadth equal to that of the groove, but of a less depth than this last. The sides which apply to each other are ground plain, so as to make them fit, and be air-tight, or nearly so; and the surfaces are made flat, in order that the whole may form one plane surface, except where the screws or springs are: these are placed below the general level of the segments. This plate, with its segments, &c. forms a covering to an endless or ring-like chamber or hollow space, which may have its cross section either a semicircle, or any other suitable figure. This covering fits close in every place but that which is occupied by the screws. Through the centre of this plate a revolving and vertical shaft passes, to which is fixed a bar or radius connected with a piston at its opposite end. To permit this bar to revolve between the edges of the chamber, and the segments which rest upon them, a roller is attached to it, and precedes it, by which each segment is raised in succession, in order to permit the passage of the bar or radius beneath it; after which it is suffered to descend again into its proper place. The chamber is also completely intercepted by a door or valve, which is capable of being moved out of the way of the piston by a revolving motion on its upper or flat side, and lodged, during the passage of the piston, in a cell adapted to the purpose. That part of the piston nearest the arbor or axis is so adapted, that any of the segments may be raised, the passage of the bar beneath it effected, and the segment again restored to its situation, before any communication can take place between the external air and the part of the chamber behind the piston.

The manner of operation is as follows: The piston being placed in any situation at a distance from the valve, and an elastic fluid introduced between them, the piston will recede from the elastic pressure, and move forward towards the opposite side of the valve; the air in the chamber passing off through the opening which contains the springs before it arrives at the valve. When the piston arrives at the valve, this last gives way, and suffers the piston to pass on; but, during the time that the valve is prevented from descending into its place again, the effect of the elastic fluid is suspended, and the motion of the piston continued by a fly, or any other well-known means: after the piston has passed, the valve descends, and the pressure operates as before; the fluid before the piston escaping by the same opening as the air in the first revolution. If steam be the elastic fluid, and a vacuum be required in that part of the chamber to which the piston is advancing, it may be effected by condensation, in the usual manner.

The patentee, in his observations, states, that his rotative piston

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is attended with much less friction than others. The space through which it will be requisite for the piston of a steam-engine of twenty horses' power to move will generally be about twenty feet. The whole weight of the segments to be raised is estimated at 400 pounds; the height to which they are to be raised in the largest engine at five eighths of an inch; and the pressure upon the piston at 4000lbs. "The shaft which goes through the centre of the engine gives motion to the air-pump, bucket, steam-valve, and all the other requisite motions, in a very simple way. There is not any thing to be seen moving belonging to these engines, but the centre shaft revolving; they do not make the least noise, take up very little room, and are only about half the price of beam-engines. I find in practice that they answer all the ends intended."

Observations.—We agree with Mr. Clegg, that he has succeeded in avoiding a part of that friction to which other rotative engines are subject; as may be seen by turning to our 1st, 2d, and 3d volumes; but we think that even this is still more considerable than he seems to suppose. The difficulty of making such an extent of surface air-tight, the liability of its parts to derangement, and the consequent want of repair, we conceive will be found to be great drawbacks upon its utility: nor has the examination of Mr. Clegg's specification effected the least change in our opinion, which induced us to state, at page 373 of our third volume, that steam-engines of the common construction were always likely to have the preference, and therefore to continue in general use. Mr. C. however, states, that he has an engine of this construction at work, which we apprehend is more than could have been said for some others for which patents were previously obtained, and which must doubtless prove the surest test of its comparative utility.

Mr. THOMAS MEAD'*s Patent for a Method or Methods of making and constructing circular or rotative Steam-engines upon an entire new Principle, and employing the elastic or expansive Force of Steam in a much more efficacious and advantageous Manner than has hitherto been done. Dated August* 1808.—*Rep. of Arts, No.* 91, *Second Series.*

MR. Mead states, that "the principle of the invention is not confined to engines of any particular construction, but consists in making use of two moveable pistons or diaphrams, in such a manner that they alternately revolve or move round their axes or centres." To effect this, two circular plates or shells of metal are made similar in their construction, having their insides turned or made very true and correct by any other means; each of these has a flanch and a semicircular cavity formed for the reception of the

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pistons. A hollow part is also formed round the centre of each for a small circular plate to turn in; and near the edge of this recess a small groove is made to encompass it. In these grooves are posited metallic rings, and packing or wadding to cause the small circular plates to work steam-tight. On the outside of each of the metallic shells there is a hollow pipe or boss, for the reception of two spindles which pass through them. Two holes are also made through one of the plates for the insertion of pipes; one for the purpose of conveying steam into the shells, and the other of conducting it from them into a condenser. One of the shafts or spindles is made hollow, to permit the other to pass through it. To the lower part of each of these shafts a piston is fixed; and each has also attached to its upper part an arm with a friction-wheel near its outer extremity. When the pistons are put in motion, these friction-wheels work in, and give motion to, a fly-wheel, and other machinery which may be connected with it. Each of the pistons has grooves round it, to admit of packing. Some directions are then given for putting these several parts together, and erecting the engine; which are followed by a description of its operation. From this we learn, that one of the pistons being fixed in a groove between the two pipes which admit and discharge the steam, and the steam permitted to enter the chamber, the other piston, by receding from its elastic force, revolves until it approach nearly to that which is fixed, which will then be set at liberty, and begin to revolve, while the other remains fixed in its turn, and so on alternately. "Engines on this principle may be made of every requisite size, and are not to be confined to the action of steam only, but may be put in motion" by a current of air, or any other fluid, "if fly-wheels, or whatever may be substituted for them, be made to drive the pistons, by being turned upon their axles with a sufficient force."

Observations.—We conceive this to be one of the simplest rotative engines which has been yet invented; and as its parts are fewer in number, and simpler in construction, they will be less liable to be out of order than those of any other engine of the kind with which we are acquainted. A considerable extent of joining, however, is still to be made steam-tight by packing, and this will doubtless want frequent repair. The patentee, in his observations printed at the end of this specification, expresses his hope of being able "to accommodate the public with engines of a far less complex construction, on more reasonable terms, and superior in performance to any that have yet been erected, and at least equal to any that may." How far the last clause but one of this sentence may be correct we have no means of deciding, as Mr. Mead has not accompanied the assertion with either calculation or experiment. We know very well, that it is frequently much easier

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to state such a proposition than to prove its truth; and we should, therefore, have been glad to have been made acquainted with Mr. M.'s mode of doing the latter as well as the former. The last clause of the sentence above quoted is certainly a very bold assertion, and is equivalent to his saying,—I have attained the utmost possible limit of invention—my engine embraces perfection—and therefore none can surpass me! We would remind Mr. M. that the progress of knowledge is slow and gradual, and that the present stock, of which mankind are in possession, and by which they are enlightened, is not the supernatural communication of a moment, but the accumulation of ages: and we would ask him, who can set bounds to the powers of the human mind but He who gave them? or who can prescribe limits to the knowledge resulting from their well-regulated exertions, except He who said to the ocean, "Thus far shalt thou go, and no further?"

Mr. James Grellier's Patent for a Building of a peculiar Construction, for the Purpose of burning Coke and Lime, whereby the superfluous Heat of the Fire used in burning the Coke is applied to burn the Lime, and also whereby such Fire may be rendered perpetual. Dated February 1809.—Rep. of Arts, No. 89, Second Series.

This building the patentee styles *the union and perpetual kiln*; and this he constructs so as to contain different compartments or chambers, adapted to the different purposes for which they are required; the space in which the coke is burned being distinct from that in which lime-stone is converted into lime. One (or more, if necessary) of these compartments is made of the form of a common coke-oven, or of any other convenient form, with its usual flues: these flues are conducted into the compartment containing the calcareous matter, in order that the heat from the coke-oven may convert it into lime. In this construction it is necessary that the heat from these flues proceed upwards through the lime-stone with sufficient rapidity and effect; and hence the lime-kiln should be situated above the flues. The said flues may either enter the lime-kiln through the bottom or sides. In the former case the lime is supported at a small distance from the bottom by an arched grating or support, constructed either of iron bars or of brick-work; and the lime drawn from the kiln by doors just above this support. When the flues enter at the sides, the places are some feet above the bottom, and the lime is drawn out in the same manner as in common kilns. With respect to the particular construction and position of these compartments, the

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patentee observes, "I do give the preference to coke-ovens of an elliptical form, so disposed in the lower part of a circular building that the longest diameters of the ellipse may respectively lie in radii of one of the horizontal sections of the said building; and the mouths of the said ovens may be open externally, and the flues may communicate internally with one lime-kiln, having the axis thereof perpendicular to the horizon, and coincident with the axis of the building itself."

Observations.—This method of Mr. Grellier's appears well adapted to effect a saving of the fuel generally used in converting calcareous matter into lime. To have this latter done effectually, much will depend upon the proportion which subsists between the quantities of coke and lime burned at the same time. We apprehend, however, that it will only answer for the lighter kinds of coke, as those which are of a ponderous nature, and of so much importance in those manufactories where an intense heat is necessary, though made from the same coals, require a different process. We know not from what inducement Mr. G. prefers the elliptical to the circular form of the coke-oven. At several places in the north of England, where the refuse small coal is burned into coke, and we may particularize the Duke of Norfolk's colliery near Sheffield, the most approved form of the ovens is the following: Each is a circular building of ten feet in diameter; the perpendicular wall is about twenty inches high; and the covering is an arch which rises about three feet and a half above this, and forms nearly a cone, with an open apex of two feet in diameter, left for the purpose of supplying the oven with coal, and to answer the end of a chimney during the whole as a part of the process.

CHEMISTRY AND MINERALOGY.

New analytical Researches on the Nature of certain Bodies: being an Appendix to the Bakerian Lecture for 1808. *By* HUMPHRY DAVY, *Esq. Sec. R. S. Prof. Chim. R. I.—Phil. Trans. Part II.* 1809.

1. MR. Davy has found that the inflammable gas, given off by the action of potassium on ammonia, possesses the same properties as hydrogen. His experiments to determine the query, whether nitrogen has a metallic basis that alloys with the metals employed, are all negative.

Numerous experiments, of the same kind as those described in

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the last Bakerian lecture (vide Retrospect, No. xx. p. 310) on the action of potassium on ammonia, were attended with similar results. When the gas afforded by the distillation of the fusible substance, was collected at different periods, it appeared that the proportion of hydrogen to nitrogen increased as the operation advanced: thus 7 measures of the first portion of gas, inflamed with $4\frac{1}{2}$ of oxygen, left 4; and $4\frac{1}{2}$ measures of the fourth and last portion, inflamed with $3\frac{3}{4}$ of oxygen, left $2\frac{1}{2}$.

By distilling the fusible substance with a fresh quantity of potassium, Mr. Davy obtained a still greater diminution of nitrogen and increase of hydrogen.

To insure the absence of metallic oxide, a tube of copper was substituted for that of iron to perform the distillation in. In one experiment, which was more depended upon than the others, the quantity of gas afforded during the first part of the operation was equal to 11 cubic inches, of which $\frac{4}{5}$ of a cubic inch were ammonia; 11 measures of the residue, detonated with 8 of oxygen, diminished to 8 :. only 2 cubic inches more of gas were produced; 10 of this gas and 8 of oxygen detonated and diminished to 10. The tube before distillation was filled with $2\frac{1}{2}$ cubic inches of hydrogen, and the other part of the apparatus contained $\frac{7}{10}$ of common air. At the end of the process there remained in the whole apparatus $1\frac{1}{10}$ cubic inch of gas. The potassium revived, acted on by water, produced $4\frac{1}{2}$ cubic inches of hydrogen. A little gray powder, that with moisture gave off ammonia, was found in the upper part of the stop-cock adapted to the barrel.

Mr. Davy assigns, for the difference between the results of this experiment, and all the preceding ones in which iron barrels were used, two probable causes: one, the superior attraction of copper for potassium might prevent the latter metal from rising in vapour and acting on the nitrogen; the other, a combination of the basis of ammonia might take place with the copper, as ammonia has a strong affinity for oxide of copper.

In consequence of Dr. Henry's opinion, which he has since corrected in a paper, of which we have given an account in our present Number, that the quantity of nitrogen in ammonia is underrated in the Bakerian lecture for 1807, Mr. Davy repeated the analysis of ammonia, and obtained precisely the same results as he did before.

Though by following Dr. Henry's method of applying hygrometrical tests, more moisture appeared to Mr. Davy in the vessel after the electrization of ammonia than before, yet he thinks that, from the nature of the alkali, this appearance is no evidence of loosely combined oxygen.

The analogy between ammonia and the other salifiable bases is determined by the amalgam from ammonia. Should the decomposition of nitrogen be proved, it would then appear that hydro-

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gen and nitrogen are different combinations of ammonium with oxygen or water.

2. On the idea that sulphur and phosphorus are deoxygenated by potassium, the substances produced by the decomposition of the sulphuret and phosphuret of this metal were examined. The substance from the sulphuret was of a dark gray colour, derived possibly from a film of naphtha, rather lighter than sulphur, but in other respects similar to it.

The substance from the phosphuret differed from phosphorus in being much deeper coloured, perfectly opake, and very brittle.

The violent combustion that takes place in combining potassium with sulphur and phosphorus has been an obstacle in the way of any accurate experiments on these bodies; but by comparative experiments Mr. Davy has ascertained that they are not analogous to the hydrogenated sulphur of Berthollet, or to the alcohol of sulphur of Lampadius. Mr. Davy has found, that the liquor of Lampadius contains carbonaceous matter only when imperfectly made; charcoal is used to procure it; and that the same charcoal will, till it is entirely consumed, answer equally for its formation. It was affected by the electric spark just like common sulphur. It absorbed oxymuriatic acid gas, and deposited common sulphur, becoming a fluid similar to sulphuretted muriatic acid: but water added, immediately produced hydrat of sulphur and muriatic acid gas.

Mr. Davy is of opinion that the liquor of Lampadius contains less oxygen than common sulphur.

3. Charcoal, though intensely ignited in nitrogen for a quarter of an hour by means of the Voltaic battery, is only rendered harder in a slight degree at the points exposed to the heat.

Red-hot charcoal ceases immediately to burn, on being immersed in oxymuriatic acid gas; and no change is produced either in the charcoal or gas, by the intense ignition of the one in the other for nearly an hour, by the Voltaic battery.

4. The results of Mr. Davy's experiments, detailed in the Bakerian lecture (vide Retrospect, p. 318), prove, that muriatic acid gas is a compound of an unknown substance, and from one third to one fourth its weight of water; that oxymuriatic acid gas contains the same substance united to oxygen, and that there is sufficient oxygen in both these gases, independent of the unknown substance, to oxidate the bodies with which it combines. This unknown substance, the dry muriatic acid, has the strongest and most extensive powers of combination: it unites with almost all bodies that have been experimented upon, with a very few exceptions.

All Mr. Davy's attempts to procure it in an uncombined state have been unsuccessful, as the decomposition of the muriats never takes place without the intervention of water. Mr. Davy has the

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greatest hopes of success in the combustion of potassium in the phosphuretted muriatic acid.

Observations.—The inquiry concerning the action of potassium in ammonia, though some new facts are brought to light, yet remains in an unfinished state; but we have no doubt that the skill and perseverance of Mr. Davy, into whose hands it has fortunately fallen, will, before long, bring it to some important termination.

The question respecting the compound nature of nitrogen rests as before, since none of the new experiments either invalidate or oppose those in the Bakerian lecture, on which it depends.

Mr. Davy has in this memoir more fully stated the conclusions which followed from his former experiments concerning muriatic and oxymuriatic acid gas. We trust that the hopes he entertains of decomposing it will be realized.

On electro-chemical Experiments. By Mr. G. J. Singer.—*Phil. Journ. No.* 108.

Mr. Singer, regretting the delay which the repetition of Mr. Davy's electro-chemical experiments has met with, arising principally from an ill-founded belief of the difficulties that occur, has been induced to offer the public his experience, in order to obviate them.

In experiments on the decomposition of bodies he found, contrary to the general opinion, that a battery with a low charge is more efficacious than with one of high intensity. The wire-melting power is only necessary to exhibit brilliant experiments. A mixture of one part of strong nitrous acid, and ten parts of water, with the addition of a very minute quantity of muriatic acid, excites the most effective power for this purpose. Three similar batteries were charged with equal proportions of the three mineral acids; that with nitric acid fused more wire than that with sulphuric, and that with muriatic less: their action on imperfect conductors was nearly similar. At the end of fourteen hours the batteries, charged with nitric and sulphuric acid respectively, had lost their wire-melting power, and acted but feebly on imperfect conductors. But the one with muriatic acid melted $\frac{5}{7}$ of the wire that it did at first, and its action on water was not apparently diminished. At the end of two days it alone was active; it melted $\frac{1}{3}$ the original length of wire, and it continued to melt wire till the fourth day: its action on imperfect conductors was evident at the end of six days.

The proportions of acid to water adapted to afford the best charge for the decomposition of the alkalies, must vary according to the number of plates employed; the greater the number the lower should be the intensity of the charge. For 200 plates not

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exceeding 4 inches: from 8 to 10 ounces of muriatic acid may be allowed for every gallon of water.

Mr. Singer has found in his experiments that naphtha is decomposed sooner than the alkalies, and that conductors of silver are preferable to those of platina: he uses the alkalies not moistened. When the metallic globules do not appear, they may be expected on that side of the alkali which rests on the silver supporter. Mr. Singer has ascertained that globules may be produced with a battery of 50 plates of 3 inches, and that the metallization of the alkaline earths, and of ammonia with mercury, may be exhibited by this power. It will readily show the transfer of alkali and acid. The best method for performing the experiment is, to connect two wine-glasses full of the tincture of red cabbage, by cotton or bibulous paper, and to add to the tincture sufficient alkali and acid to neutralize each other: on making the communication with the two poles of the battery, the fluid in the negative cup will become by the attraction of the alkali of a beautiful green colour, and at the same time the fluid in the positive cup assumes a fine red colour: by changing the cups when the effect is produced, the green colour in the one will gradually vanish, and appear in the opposite vessel; and the red colour will take the place of the green. This alternate transfer may be repeated several times with the same charge, and may be produced by a trough of only thirty double plates of two inches square.

Observations.—Mr. Singer, we think, feels unnecessary regret, as Mr. Davy's experiments alluded to have been successfully repeated by the best chemists of our own, and of foreign countries. His directions, perhaps, may be of service to those who are unacquainted with the Bakerian Lecture of Mr. Davy.

On expectorated Matter. *By* George Pearson, *M. D. F. R. S.* —*Phil. Journ. No.* 108.

Dr. Pearson distinguishes seven varieties of expectorated matter:—

1. The jelly-like semi-transparent kind, of a blueish hue, secreted in a healthy state.

2. The thin mucilage-like transparent matter so copiously expectorated in bronchial catarrhs.

3. The thick, opake, straw-coloured, or white and very tenacious matter, coughed up in a great variety of bronchial and pulmonary affections, especially in that of tubercles.

4. Puriform matter secreted without any breach of continuity, or breach of surface of the bronchial membrane; very commonly occurring in pulmonary consumptions.

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5. The matter which consists of opake, viscid masses, together with transparent fluid.

6. Pus from the vomicæ of tubercles.

7. Pus from vomicæ by simple inflammation of the lungs, and without tubercles.

Of these varieties Dr. Pearson has confined himself in this paper to the first five, which he finds do not in any respect differ from each other, but in the proportions of their ingredients.

Expectorated matter consists of albuminous animal substance, to which Dr. P. has applied the name oxide, from its neutralizing power; and water impregnated with several saline and earthy bodies. The proportion of animal oxide is various; generally between five and six per cent. of the expectorated matter.

The substances impregnating the water are,—muriat of soda, varying generally from $1\frac{1}{2}$ to $2\frac{1}{2}$ per 1000 of expectorated matter; potash, from $\frac{1}{2}$ to $\frac{1}{4}$; phosphate of lime about $\frac{1}{2}$; ammonia probably united to the phosphoric acid; phosphate, perhaps of magnesia; carbonate of lime; a sulphate; vitrifiable matter, or perhaps silica; and oxide of iron:—the last six substances scarcely amounting to 1 part in 1000.

The quantity of saline impregnation in the expectorated matter is proportionable to the fluidity; the thicker it is, the more animal oxide it contains.

According to Dr. Pearson's experiments, all the human fluids contain potash neutralized by an oxide of animal matter. He thinks it most probable that the circulating and secreting fluids are impregnated with potash neutralized by an animal oxide or acid, as his experiments give reason to believe; and not with soda, according to the generally received opinion.

Expectorated matter belongs to the class of coagulable, and not of gelatinizable fluids; and it differs from serum of blood in forming a thicker fluid with a larger proportion of water.

Dr. Pearson observed in the opake ropy kind of expectorated matter, as well as in the puriform, minute globular particles, similar to those in blood and pus, which were indestructible, except by such agents as destroy charcoal. Dr. P. ventures the question, whether these particles do not chiefly consist of organized carbonaceous matter.

Observations.—The fact of most importance established by Dr. Pearson's experiments is, the existence of potash in animal fluids. His labours no doubt will be of much service to physicians, and the cultivators of animal chemistry; for they afford the means of distinguishing between pus and mucus, accurate tests of which have been long wanted.

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On a native Arseniat of Lead. By the Rev. WILLIAM GREGOR. —*Phil. Trans. Part II.* 1809.

THIS mineral was found in Huel-Unity, a copper-mine in the parish of Gwennap, Cornwall. It is crystallized; its most perfect crystallization is a hexaedral prism, small, or very small. Some of the crystals are hollow. The gangue is white quartz. The colour of the crystals consists of a variety of tints: of yellow; wine-yellow, passing into a delicate Isabella colour; and honey-yellow, mingled with brown of different intensities. Some of the crystals are transparent; others only partially, or semi-transparent. The external lustre in some specimens is vitreous; in some resinous; and others are partially covered with fine filaments of a silky lustre. The angular fragments of the mineral are sufficiently hard to scratch glass. The specific gravity of the purest crystal was 6.41.

Mr. Gregor could only partially decompose this arseniat by boiling it to dryness in a solution of four times its weight of subcarbonate of potash, and by igniting the dry mass for a short time.

He found that it was precipitated unaltered from its solution in nitric acid by carbonate of ammonia.

In ascertaining the quantity of muriatic acid that this mineral contains, he detected two sources of error: one, that the muriatic acid was apt to be expelled in vapour by the action of nitric acid; another, that a simultaneous precipitation of arseniat of lead takes place with muriat of silver.

In his comparative experiment he observed that arsenic acid forms a triple salt with ammonia and magnesia.

Mr. Gregor has found, that the component parts, and, in most cases, the proportions, are the same in all the specimens he has examined. According to his experiments, 100 of this ore contains 69.76 of oxide of lead; 26.40 of arsenic acid; and 1.58 of muriatic acid. One specimen in a more compact state only differed from this: it consisted of 71.46 of oxide of lead; 23.88 of arsenic acid; and 1.63 of muriatic acid.

Observations.—Mr. Gregor has conducted the analysis of this new mineral with that ingenuity and attention to accuracy which always characterizes his labours. We hope that the discoverer of titanium will continue to direct the attention of English mineralogists to his native county, that rich storehouse of British minerals, which has been too long neglected.

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Observations on Albumen, and some other animal Fluids; with Remarks on their Analysis by electro-chemical Decomposition. By Mr. WILLIAM BRANDE, *F. R. S.—Phil. Trans. Part II.* 1809.

MR. Brande's experiments were first directed to ascertain how far mucus in its pure state was capable of conversion into modifications of gelatine and albumen.

As the re-agents employed to detect mucus were found to act principally on the salt it contains, it became desirable to separate these without affecting the mucus. The Voltaic battery was conceived to afford the best means of attaining this object.

Three small glass cups, one containing equal parts of saliva and water, and the other two pure water, were acted on by a battery of 120 four-inch plates. After about ten minutes, a quantity of white matter was deposited on the cotton at the negative surface, but no such effect occurred at the positive. As Mr. B. could not readily account for this coagulation, the albumen of an egg was electrified with a view to elucidate it: an immediate and rapid coagulation occurred at the negative, while only a thin film of albumen appeared at the positive wire. Mr. Davy explained the fact on the idea that the fluidity of albumen depends on the presence of alkaline matter, and its separation at the negative pole is the cause of its coagulation. Dr. Thomson also, in his "Chemistry," offers a very similar explanation:—subsequent experiments seem to confirm this opinion.

Water in which coagulated white of egg had been boiled, was found to be a very diluted alkaline solution of albumen. The brown viscid fluid which separates in minute quantities from pieces of coagulated white of egg, is also principally an alkaline solution of albumen.

The white of egg in its fluid state appears to be composed of albumen, alkali, and water: by heat the alkali separates from the albumen, combines with the water, and hence occasions coagulation. These facts also explain the coagulation of albumen by alcohol and by acids, and why it is more perfect by acids than by alcohol.

By electrization and subsequent evaporation albumen furnished soda nearly pure, and muriate of soda. Though in the experiments detailed from 60 to 300 double plates had been used, it was found that twenty-four three-inch plates effected a perfect coagulation, even in albumen so diluted as not to be detected by the usual tests.

Saliva, some varieties of mucus, bile, milk, the liquor of the amnios, and pus, were examined by the battery. Saliva, mucus from the oyster, trachea, nose, &c. copiously coagulated at the

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negative pole, even in cases where the usual tests indicated no albumen; and it was found by minute experiments, that the alkaline and acid matter evolved from saliva, mucus of the trachea and nose, were soda and muriatic acid, with traces of lime and phosphoric acid. The mucus of the oyster afforded only soda and muriatic acid. The proportion of alkali appeared to predominate, though in the original fluids no traces of uncombined alkali (as in white of egg) are to be detected. These results lead to no new views relative to the composition of mucus. Is it a combination of muriate of soda and albumen? or of soda and albumen, from which the alkali can only be separated by the battery?

Bile, when electrized, immediately coagulated; the albumen was tinged green, from the colouring matter separated. The albumen obtained from bile, varied from 0.6 to 8 per cent.; and it is singular, that, when the albumen is in small quantity, the resinous matter is relatively small. It afforded a quantity of soda and muriatic and phosphoric acids.

When milk is electrized, albumen after some time separates, the appearance of curd and whey is produced, little of the curd collects at the positive pole. Soda, with traces of lime and muriatic and phosphoric acids, is obtained, and the serum still affords sugar of milk.

The liquor of the amnios, in its pure and fresh state, was examined by electricity; it afforded albumen, soda, and muriatic acid.

Pus, when electrified, coagulated at both poles, but more abundantly at the negative; a slight degree of putrefaction occurred: the other products were not noticed.

The decomposition of liquid albumen by Voltaic electricity seems to be regulated by the power employed. With a pretty high power, the coagulation is rapid at the negative pole, and very slow at the positive; but with a very low power it is comparatively rapid at the positive, while an alkaline solution of albumen surrounds the negative. Hence a quick abstraction of alkali seems necessary to the perfect coagulation of albumen.

Observations.—Hitherto the Voltaic apparatus has scarcely been applied in the examination of animal fluids; but since Mr. Davy's enlightened researches, the true powers of this important instrument have been more justly appreciated, and we may now confidently hope that this part of the science will receive considerable elucidation.

The cause of the coagulation of albumen in various circumstances was never so clearly explained, as by these experiments of Mr. Brande; and he has also confirmed the ingenious conjectures of Mr. Davy and Dr. Thomson on the subject.

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Experiments on Ammonia, and an Account of a new Method of analyzing it, by Combustion with Oxygen and other Gases. By WILLIAM HENRY, *M. D. F. R. S.—Phil. Trans. Part II.* 1809.

MANY experiments were made by Dr. Henry with a view to detect the presence of oxygen in ammonia. The gas was electrified, from an idea that oxygen might possibly be evolved in a separate state, but without success. It was conceived that water might probably be formed in the same process. To ascertain whether this was the case, very pure ammoniacal gas, which had been exposed for some time to dry potash, and which did not deposit moisture by a cold of 0 Fahr. was strongly electrified. In the first trials of this kind there appeared to be water formed, for the gas which was not affected by a freezing mixture, previous to the process, now gave evident signs of moisture. But on repeating the experiments with additional precautions, there were no signs of moisture at 20° Fahr. and scarcely any by a cold of 0 Fahr. or a few degrees below. Hence Dr. H. suspects that the moisture in the early experiments was not generated, but derived from the mercury.

Ammonia strongly absorbs and retains moisture. Common air may be dried in a few minutes by pure potash or muriate of lime, so that no ice appears when the vessel is exposed to 20° Fahr. But ammonia requires some hours exposure to stand the test of 0 Fahr. and a single transfer of the dried gas through mercury in ordinary use communicates moisture to it.

In the decomposition of ammonia by electricity, results similar to those stated by Berthollet were obtained; but when the mercury was previously boiled in the tube, and warm when the gas was introduced, less gas was obtained, viz. at the rate of 180.6 from 100. Recent experiments have convinced Dr. H. of the accuracy of the proportions assigned by Mr. Davy, viz. 74 hydrogen to 26 nitrogen. The nearest approximation he has made to these numbers is 73.75 to 26.25.

The extreme labour attending the decomposition of ammonia by electricity, induced Dr. H. to adopt new methods.

Oxygen gas affords a ready and precise means of analyzing ammonia; when mixed in proper proportions they may be fired by an electric spark over mercury. If more than 3 of pure oxygen to 1 of ammonia, or 3 of ammonia to 1.4 of oxygen be used, the mixture does not detonate. Common air does not inflame with ammonia, though, by long continued electrization with air, ammonia is at last decomposed; and when the gases are previously dried, the production of water is apparent by either. Hence, sup-

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posing oxygen to exist in ammonia, it cannot be obtained in a separate state by electricity.

The analysis of ammonia by oxygen, varies according to the proportions of the gases employed. If the volume of oxygen be double that of the ammonia by detonation, only nitrogen and the redundant oxygen remain; a dense cloud is seen, which forms a white crust on the tube; or if the cloud is not readily visible, by agitating the mercury in the tube, it appears. This is probably owing to some ammonia which had lodged in the mercury. The white matter resembles nitrate of ammonia; but when the excess of oxygen is removed by sulphuret of lime, less nitrogen is obtained, than the ammonia should have furnished. Hence ammonia can scarcely be accurately analyzed when a considerable excess of oxygen is used.

On the contrary, when the ammonia is in much excess, no nitrous acid appears; and it is remarkable, that though the oxygen be not sufficient to saturate the hydrogen, yet the ammonia is entirely decomposed. By adding more oxygen, detonation may be again effected, and in this way all the hydrogen may be condensed into water while the nitrogen only remains. The quantity of oxygen consumed, being known, the hydrogen may be easily calculated, being (when no nitrous acid is formed) pretty exactly twice the volume of the oxygen used.

In one experiment, 66 measures of ammonia were exploded with 33 of oxygen containing one of nitrogen; they diminished to 57, and no farther diminution occurred with water and sulphuret of lime; all the ammonia was therefore decomposed, and all the oxygen had combined with the hydrogen. To these 57 measures, 40 of oxygen were added; they diminished to 60. Hence, by calculation, these would give 73.88 hydrogen to 26.12 nitrogen.

The results from numerous experiments were not perfectly uniform, but it can scarcely be doubted that this want of coincidence was owing to the variable state of dryness of the ammonia. In an experiment which Dr. H. considers as the most accurate, 100 measures of ammonia gave 73.5 hydrogen and 26.5 nitrogen. The quantity of oxygen used was to that of the ammonia as 67.5 to 100; and these proportions appear to be the best for mutual saturation.

By means of an apparatus on a plan described in Phil. Trans. for 1808, ammonia expelled from the orifice of a small steel burner may be kindled by electricity in oxygen gas. It is slowly consumed with a pale yellow flame. The combustion is however too feeble to be useful in the analysis of ammonia.

Nitrous oxide may be used with success in the analysis of ammonia. Any mixture of these gases in which the ammonia is not less than $\frac{1}{6}$ of the whole, may be fired by electricity: a dense cloud appears, having sometimes an orange colour. When the

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nitrous oxide much exceeds the ammonia, as 100 to 30, there is scarcely any diminution after detonation, and the residuum consists of a little undecomposed oxide, some oxygen and much nitrogen, though not in its full proportion. When the nitrous oxide is yet increased, more oxygen is found in the residuum.

On the contrary, when the ammonia is redundant, combustion does not occur unless the nitrous oxide forms $\frac{1}{3}$ of the mixture; there is a slight diminution by firing, but no cloudiness. In an experiment, 41 measures of ammonia and 40 of nitrous oxide (= 38 pure) were reduced by detonation to 75, which consisted of 16 hydrogen and 59 nitrogen. These, by calculation, would give the proportions of hydrogen and nitrogen as 75.4 to 24.6. From the same facts it may be deduced that 100 measures of ammonia require for saturation 130 of nitrous oxide = 67.5 of oxygen. The coincidence then between the analysis of ammonia by nitrous oxide and by oxygen, confirms the accuracy of both methods.

Nitrous gas detonates with ammonia. The proportions necessary for mutual saturation are about 120 of nitrous gas to 100 of ammonia. An excess of the former gives inaccurate results; as not only the hydrogen of the ammonia, but also some of its nitrogen is condensed, and the mixture after detonation assumes its usual cloudy appearance. In the results with nitrous gas however, some slight differences have occurred, which Dr. H. has not yet been able to reconcile.

Dr. Henry briefly states the results of some experiments made in conjunction with Mr. Dalton on the effect of electricity on the aëriform compounds of carbon and hydrogen. Subsequent reflection, and the candid and judicious remarks of others, have led Dr. H. to doubt the accuracy of some conclusions drawn from former inquiries. (Phil. Trans. vol. 77.)

It appears from these experiments, that the hydro-carburetted gases are decomposed by long electrisation, the carbon is precipitated, and the hydrogen evolved in an expanded state. Carbonic acid is also converted by the same process into carbonic oxide, and oxygen; and when the unaltered carbonic acid is removed, those gases inflame by the electric spark, and again reproduce carbonic acid. Carbonic oxide must retain its carbon with a very strong affinity, as 1100 discharges from a Leyden jar on $\frac{1}{10}$ of a cubic inch of this gas, produced no change.

Observations.—The most accurate of Dr. Henry's experiments prove what Mr. Davy had before stated in his Bakerian lecture, that in the analysis of ammonia by electricity there is uniformly a deficiency in the products obtained. Dr. Henry does not enter into the inquiry whether the presence of oxygen may not occasion this loss.

The most satisfactory evidence relative to the existence of oxy-

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gen in ammonia seems to arise from the circumstances attending its metallization. If there be any truth in the antiphlogistic theory, ammonia must be an oxide. At present, however, as the great problem concerning the nature of hydrogen and nitrogen is yet unsolved, it would be premature to form any conclusions on the subject.

Hitherto no attempts seem to have been made to analyze ammonia by any other gases, though this alkali has engaged so much of the attention of philosophical inquirers. Dr. Henry has, with his usual ability, applied oxygen, nitrous oxide, and nitrous gas for this purpose; and the near coincidence of the results, and their agreement with Mr. Davy's experiments, seem to prove the accuracy of the methods.

On the Identity of Columbium and Tantalum. By Wm. Hyde Wollaston, *M. D. Sec. R. S.—Phil. Trans. Part II.* 1809.

Soon after the discovery of columbium in 1801 by Mr. Hatchett, tantalum was discovered by M. Ekeberg. It has been doubted whether these are distinct metals. With a view to ascertain this point Dr. W.'s experiments were instituted.

It is very difficult to discern any characteristic difference between columbite and tantalite. Their external characters are exactly alike: columbite is rather more easily broken, and its fracture is less uniform. By analysis also, they yield the same substances, viz. a white oxide with iron and manganese. The white oxide from both is nearly insoluble in the three common mineral acids. Its proper solvent, as Mr. Hatchett and M. Ekeberg have observed, is potash. It is also entirely dissolved by oxalic, tartaric, and citric acids.

The characteristic precipitant of columbium is the infusion of galls; but in the use of this test, an excess of potash, or oxalic or tartaric acid should be avoided, as it will render the results inaccurate.

The perfect agreement of these oxides in their chemical properties, seems clearly to establish their identity. There is, however, a remarkable difference in the specific gravity of the minerals from which they are procured. That of columbite, according to Mr. Hatchett, is 5.918, and that of tantalite, as stated by M. Ekeberg, is 7.953. Dr. W. found the specific gravity of tantalite, in four separate fragments, 7.8, 7.65, 7.58, and 7.15, and that of columbite 5.87.—Columbite contains $\frac{1}{4}$ of oxide; but it is evident that no variation of mere proportion can account for an increase of mere specific gravity from 5.918 to 7.953. This circumstance may arise from a different state of oxidation; also, in part, from cavities in the columbite, and from its aggregation.

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Observations.—The experiments of this able chemist seem fully to establish the identity of columbium and tantalum.

Probably, when some of the other new metals shall have been submitted to a rigorous examination, they will meet a similar fate.

Process for making sublimed Muriate of Quicksilver (Mercurius dulcis or Calomel); with a Method of purifying the common Sort of it. By M. PLANCHE.—*Ann. de Chim. May* 1808.

A MUCH surer and more simple method of preparing calomel than the common, is to sublime sulphate of quicksilver ad minimum with dried muriate of soda.

To prepare the first, three parts of sulphuric acid at 66° Beaume are boiled upon two of quicksilver in a retort. After the operation, the retort is either broken, or the saline mass is separated by means of a bent rod, which is easily performed.

The acid sulphate thus obtained, is very friable and very white; but it is changed to a yellow colour, by the smallest quantity of cold water. In order to form from it the sulphate of quicksilver ad minimum, 18 parts of it are ground with 11 of quicksilver, adding by little and little 6 parts of cold water. At first the mass becomes yellow, heat is evolved, and then the mass changes to a deep gray; the trituration is continued until the mixture becomes of a dirty white, and the quicksilver entirely disappears. It is then dried at the temperature of 30 to 35° Réaum. or 100 to 110° Fahr.

This sulphate is soluble in distilled water, and the solution alters neither tincture of litmus, nor syrup of violets, but it is precipitated black by lime-water, and gray by ammonia.

An equal weight of this sulphate and dried common salt being sublimed, yields calomel as white as the common sort, and purer than that which comes from the Swiss manufactories; 4lb. of the mixture usually yields about 30 ounces of calomel.

In case of impurity, or of the heat not being well managed, calomel is to be pulverized extremely fine, put into a flat-bottomed matrass, and covered with a stratum, 2 lines (1-6th inch) thick of fine sand, which has been previously washed with water, acidulated by muriatic acid. The calomel is thus to be re-sublimed, when it will be rendered very pure.

On some of the principal Substances found in a Collection of the Lava of the Vicentine Alps. By M. TONNELIER.—*Journal des Mines, No.* 128.

MR. Brard supposed that the blue substance found in these lavas was a blue laminar chabasie; but it yields, by mechanical

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division, straight rhomboidal prisms, whose angles are respectively 105° and 75°, which is similar to that of sulphated strontian. It has also the same difference between the natural polish of the bases and of the sides of the prism, and the same diagonal joints were distinctly recognised. This determination was confirmed by its being found by M. Berthier to contain strontian and sulphuric acid.

The sarcolite of Thomson, which is also found in these lavas, is considered by Hauy as a flesh-colour variety of analcime; but Vauquelin is of opinion that it is a particular species, and that it should be placed by the side of analcime among alkaliferous stones (see p. 119 of the present vol.); the difference, however, that is found in their constituent parts, is not greater than those of the various sorts of topaz, and no more stress can be laid on the slight variations between the properties of the two minerals. In reality, if several minerals differ in the proportion of their constituent parts, but agree in their physical properties, they ought to be esteemed as a single species; and on the other hand, if minerals agree, like carbonate of lime and arragonite, in the proportional quantity of their ingredients, but differ essentially in physical properties, they ought to be considered as separate species. Upon this account, chabasie, which has been found by Vauquelin, who only has as yet analyzed it, to contain 43.33 per cent. of silica, 22.06 of alumine, 3.34 of lime, 9.34 of soda mixed with potash, 21 of water, and some traces of iron and manganese, and which of course contains the same ingredients as analcime and sarcolite, but in different proportions, ought, from the differences observed in its crystalline forms, to be considered as a separate species, but placed near the other.

This lava also contains mesotype in several forms; apple-green carbonate of lime in rhomboids nearly approaching to a cube, and which might be mistaken for chabasie; large-grained carbonate of lime imbedded in the volcanic tophus, and containing marine shells, or their impressions, which are sometimes lined with analcime and sulphate of strontian; a green earth filling the cavities of amygdaloid; carbonized fossil wood; and a red stilbite that appears similar to the fassoite of Dolomieu.

On dead Lime. By Mr. Bucholz.—*Journ. des Mines, No.* 129.

In certain circumstances carbonate of lime is changed by burning into a lime which does not heat with water, nor slake, and which is called *dead lime*.

There are four cases in which dead lime may be formed: 1. When the limestone contains much alumine, and it has been heated, so as to harden the mass. 2. When the limestone contains silica, and it has been strongly heated after the expulsion of

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the carbonic acid. 3. When the limestone is so strongly heated as to be in some measure melted, in which case the carbonic acid is frequently, not entirely separated from the central parts of the large lumps, and they of course effervesce with acids. 4. When the limestone is strongly heated after the total separation of the carbonic acid, so that the lime neither splits when water is added to it, nor does it effervesce with acids.

Dead lime has been formed from chalk or oyster-shells, but the circumstances from which it originated have not been discovered. It grows very hot with dilute muriatic acid, without emitting any carbonic acid gas. It remained 24 hours in water without slaking, but formed lime-water. When calcined oyster-shells were put into a boiling ley of soda, they were completely decomposed and formed a very fine cream.

On artificial Sulphuret of Zinc. By A. G.—*Journ. des Mines, No. 129.*

Five grammes of sublimed zinc and as much flowers of sulphur were mixed together, and heated in a crucible; the metal was left alone, and no combination had taken place.

A similar mixture of zinc and sulphur was put into a lined crucible covered with powdered charcoal, and heated. When ignited, an explosion took place, much zinc was burned, and a white substance was left, slightly agglutinated together, which dissolved in nitric acid with effervescence, and the separation of much sulphur.

A similar mixture was put into a crucible lined with sulphur, and covered with sulphur. On heating it, a violent explosion took place; but 3 gr. of yellowish white sulphuret of zinc was left, which yielded sulphuretted hydrogen by the action of muriatic acid. This experiment was repeated with the same result.

A double quantity of the materials was heated in a glass retort. The sulphur melted, a part of it sublimed, and the zinc began to collect at the bottom of the retort; a slight explosion with red light took place, and then another which burst the retort.

A similar explosion takes place when copper is combined with sulphur, and Descostils has also observed the same in alloying zinc with platina.

The sulphuret obtained does not contain any sulphate, and appears to contain 30 per cent. of sulphur, and 69 of zinc; so that it does not seem to differ much from native sulphuret of zinc, but differs greatly from the artificial sulphuret, as stated by Proust.

Observations.—The paper on blende, written by Prof. Proust, and to which this paper refers, is abstracted in p. 255 of the present volume.

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On the Muriates of Barytes and Silver. By M. BERTHIER.—*Journ. des Mines, No.* 130.

IN a former memoir on the sulphates of barytes, &c. (p. 252), it was taken for granted, that muriate of silver contained 20 per cent. of muriatic acid, and the composition of muriate of barytes was deduced from this statement; new experiments have shown this to be erroneous.

10 g. of barytes newly prepared from the calcined nitrate dissolved in water, left 0.4 of carbonate; the solution was saturated with muriatic acid, and, evaporated to dryness, it left 12.75 of calcined muriate; so that this salt is composed of 75.3 per cent. of barytes, and 24.7 of muriatic acid. 10 gr. of crystallized muriate of barytes lost 1.5 by calcination, to that the crystals contained 64 per cent. of barytes, 21 of muriatic acid, and 15 of water.

5 gr. of calcined artificial muriate of barytes, containing 1.235 of acid, being precipitated by nitrate of silver, threw down 6.75 of muriate of silver; so that this muriate contains 18.3 per cent. of muriatic acid; and as it contains 75 of silver, there remains of course 6.7 of oxygen. This nearly agrees with the statement of Proust, Journ. de Phys. xlix. 221.

8.5 gr. of calcined muriate of barytes precipitated by sulphate of soda yielded 9.55 of sulphate of barytes: hence this salt contains 34.5 per cent. of acid, which differs very little from the former statement.

Observations.—The proportions of the ingredients in barytic salts, as here stated, do not exactly agree with those given by Mr. Aikin, an abstract of whose paper may be found in p. 229 of this volume. This shows that further experiments must be made in order to explain the cause of these differences.

On vegetable Astringents. By J. BOSTOCK, *M. D.—Phil. Journ. No.* 108 *and* 109.

WHILE Dr. B. was engaged in a series of experiments on tan and jelly (Retrospect, No. 20), he tried the effects of various re-agents on the astringent substances he employed.

The results are detailed, and differ in some respects from those of other able experimenters. The observations and experiments have been confined to the gall-nut, catechu, and the extract of rhatany.

The greenish hue which some infusions of galls assume, has been attributed by M. Deyeux to a green colouring matter, and

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by Mr. Davy, to gallate of lime: this last opinion is objectionable, for, by adding lime-water in small quantities to a recent infusion, so as not to precipitate the tan, this colour is not produced; the lime, however, must engage the acid, and thus form gallate of lime. Dr. B. could not detect lime in an infusion of galls; though it may exist in the gall-nut, it is not taken up by water.

The mould, which after some time appears in infusions of galls, has been ascribed by Deyeux, Tromsdorff, and others, to mucus. If mucus exists in the infusion, it is not the immediate cause of mould. Dr. B. conceives, that the operation of moulding is connected with that part of the galls precipitated by muriate of tin, and perhaps slightly with tan. Probably by this process, and by peculiar treatment, an infusion might be deprived of all its tan, and what has been called its extract.

In the use of muriate of tin as a re-agent, it is proper to notice, that there are two varieties of this salt; they may be distinguished by the names of muriate and oxymuriate; and when properly formed, the muriate occasions precipitates in oxymuriate of mercury, nitro-muriate of gold, and nitro-muriate of platina: the oxymuriate has no effect on these salts.

The effects of these salts of tin in astringent infusions are by no means identical, though this circumstance has not been noticed *. In an infusion of one part of galls to eight of water, macerated for 24 hours, and then mixed with 50 times its weight of water, oxymuriate of tin gave a slight precipitate; but there was no effect when the water was 100 times the weight of the infusion. Care must be taken that this salt be not very much diluted, as in this case it becomes insoluble.

The nitro-muriate of tin appears to be nearly as delicate a test as the oxymuriate; and both of them are much superior to the muriate.

Dr. B. minutely observed the effects produced by subjecting quantities of galls to twelve successive infusions, at the boiling temperature, and also at the temperature of the atmosphere. The cold infusions were generally of a deeper brown colour, they acted on the re-agents longer, and began to mould sooner than the warm infusions. In the early infusions the precipitates were very copious, but their quantity gradually diminished. The oxymuriate of tin first ceased to produce any effect, soon after jelly; but oxysulphate of iron indicated gallic acid after a number of additional infusions.

Gallic acid appears to be more readily soluble than tan, though it is generally stated as being less soluble; and on this notion, Mr. Biggin founded his process for ascertaining the relative quantity of tan and gallic acid in the substances used in making leather. Tan

* Davy, Proust, and others call the salt they used muriate of tin; but from its effects it seems to have been an oxymuriate, or a mixture of both.

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and extract, though contrary to received opinions, appear to be nearly equal in solubility.

Besides the salts of tin, jelly, and oxysulphate of iron, other re-agents have been used in the analysis of galls. The principal are sulphuric and muriatic acids, fixed alkaline carbonates, aluminous salts, lime-water, and acetate of lead. The acids have been proposed as agents, to separate tan from the infusion; but they are inferior to jelly, which occasions a considerable precipitate after they have ceased to act. Sulphuric acid, as a test, is superior to muriatic acid. Nearly the same remarks apply to carbonated alkalies and lime-water. Aluminous salts have been used to detect extract; but whether they act on the tan or the extract, they are much less delicate tests than the salts of tin, which produce a precipitate where those have no effect.

Acetate of lead is the best precipitant of vegetable infusions. It completely separates all the constituents, and seems to detect gallic acid more readily than even oxysulphate of iron.

Tartarized antimony was used by M. Vauquelin in his valuable experiments on cinchona. In an infusion of galls it separates all the tan and extract, and the chief part of the gallic acid. The nitro-muriates of gold and platina produce brownish precipitates in infusions of galls.

Tan, gallic acid, extract, and mucus, are considered as the principal soluble parts of the gall-nut; jelly and oxysulphate of iron afford clear evidences of the two first, but those of extract are less distinct.

The principal methods used to detect extract in infusions are—1. When oxymuriate of tin occasions a precipitate after jelly has ceased to act. 2. When an insoluble precipitate occurs in an infusion exposed some time to the atmosphere, or to an increased temperature. 3. If two portions of galls be infused in water, one for a short and the other for a longer period, the first is more acted on by jelly, the other by oxymuriate of tin.

The first of these circumstances seems ambiguous; jelly seldom separates the whole of the precipitate at once from infusions: some remains suspended. The addition of jelly, after some time, occasions a fresh precipitate. When jelly has no farther effect, oxymuriate of tin will produce a slight precipitate, which cannot with certainty be referred to the union of extract with oxide of tin. The fluid may still contain jelly and tan; for in successive additions of jelly to an astringent infusion, the first quantity added unites with a large proportion of tan, and forms a compound more insoluble than any subsequent ones. When successive portions of jelly are added to infusions, more precipitate is obtained, than when what is judged a sufficient quantity is introduced at once. Comparative experiments on rhatany seem to confirm these statements.

The second method of detecting extract in infusions is ques-

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tionable. Is the insoluble precipitate merely extract? If an infusion be evaporated at a heat not above 212°, a deep brown transparent mass is obtained, which cannot be entirely dissolved. The clear solution may several times be treated in a similar way, with analogous results. This proves that the supposed extract of an infusion cannot, as has been imagined, be separated by one evaporation; and may lead to the idea, that the effect is rather on its general constituents, than on one only. The processes in which tan is rendered insoluble by the addition of oxygen, as by oxide of tin, and nitric and oxymuriatic acids, seem to strengthen this notion.

The opinion that insolubility after evaporation is a distinct character of extract, we owe to M. Fourcroy's experiments on the bark of St. Domingo; but the analogy cannot be supported, without independent facts. Dr. B. concludes that the tan is rendered insoluble in these operations.

The third mode of proving the existence of extract seems to be very defective. It is said, by subjecting astringents to hasty infusions, the tan is obtained nearly free from extract; but by continuing the infusions longer, the fluid consists chiefly of extract. Dr. B. found, in all cases, that the relative effects of jelly and oxymuriate of tin on infusions, were exactly as their strength, and that the precipitates from the first infusions are not materially different from those in subsequent ones. Indeed, when successive infusions are made, the results (as already stated) are quite opposed to received opinions.

It may be observed, relative to the distinction between tan and extract, that jelly, the appropriate test of tan, acts powerfully on extract; and oxymuriate of tin, the re-agent of extract, exerts a similar action on tan. The facts stated, show an intimate connexion between these bodies; and M. Vauquelin, in enumerating the peculiar properties of extract, equally applies them to tan.

There appears to be no known method of accurately distinguishing extract in infusions, nor has its peculiar character been yet developed. One fact favours the opinion of its existence in galls. If two equal infusions of galls be saturated; one with jelly, and the other with oxymuriate of tin, and exposed some time to air, until the deposition of all their precipitates; the fluid treated with jelly will now be precipitated by oxymuriate of tin; and that with oxymuriate of tin by jelly. But viewing the compound nature of the fluid acted on, and other circumstances, the evidence of their individual existence is not distinct.

Though mucilage is reckoned a constituent of galls, and Mr. Davy gives a process for obtaining it in a separate state, there seems to be no distinct evidence for its existence; what Mr. Davy considered as mucus in his analysis, Dr. B. refers to an imperfect compound of tan and jelly.

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Dr. B. notices the effects of water, alcohol, and different reagents, on catechu, and the extract of rhatany. Catechu is more variable in its qualities than galls. After nine successive infusions only $\frac{1}{10}$ insoluble matter remained. Dr. B. conceives that he detected gallic acid in an infusion of catechu. As the tan and extract could not be separated by any of Mr. Davy's methods, it seems that the catechu examined in the two cases must have been essentially different.

Dr. B. seems to favour the opinion of Proust, that tan is not in all cases uniform in its properties.

To those of our readers who wish to have more minute information, we beg to refer to Dr. Bostock's papers.

Observations.—We hoped, from some of the former labours of Dr. Bostock, that he would have pursued animal and vegetable chemistry with a spirit that would have promoted its extension and improvement; and in consequence, we are woefully disappointed by the perusal of these papers.

Dr. Bostock seems actuated rather by the desire of discovering error in others, than of ascertaining truth himself. Any man who labours with such views ought at least to be master of his subject; and when he intends to convict other chemists, of mistake, should guard with more than common caution against sources of error in his own reasonings and processes. Dr. B. seems to doubt whether Proust and Davy know the difference between muriate and oxymuriate of tin as tests for vegetable substances; and he has given us his own method of making oxymuriate of tin, by acting on tin by nitrous acid, and dissolving it in muriatic acid. Every tyro in chemistry must know that in this case a nitro-muriate of tin must be formed.

Dr. Bostock appears to be ignorant of the general laws of the combinations of vegetable substances, and of the changes they undergo; he has misrepresented the opinions of other experimenters, as, for instance, where he says that Mr. Davy ascribes the green colour of some infusions of galls to gallate of lime.

The author does not seem to have kept in view the well-known fact, that the composition of vegetables varies extremely, according to the nature of the soil in which they grow.

On Platina and native Palladium from Brasil. By William Hyde Wollaston, *M. D. Sec. R. S.—Phil. Trans. Part II.* 1809.

This new mineral has been lately received from the gold-mines in Brasil by the Portuguese ambassador. Its general aspect is very different from the common ore of platina. It has the spongy form of platina, imperfectly alloyed with arsenic. On minute examination, small particles of gold are perceived, but no magnetic

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iron-sand nor small hyacinths which accompany the Peruvian ore.

The Brasilian platina (unlike the common ore) has no polish, and does not appear worn; most of the grains have a spongy appearance, and even those which are entire and rounded present a roughness very different from the latter.

In treating a portion of the ore with nitro-muriatic acid, two of the fragments being more rapidly acted on than usual, and giving a redder colour than platina alone, were reserved for a separate examination. The remainder proved to be nearly pure platina, with a minute quantity of palladium, which these separated grains might have furnished.

In three of the largest grains of ore a quantity of gold, not exceeding $\frac{1}{100}$ of a grain, was found. The Peruvian ore itself does not contain the smallest quantity of gold. Though neither the Peruvian nor Brasilian grains of platina contain any silver, the gold accompanying them is alloyed with it. Some small scales of gold, picked from Peruvian platina, were found to contain $\frac{1}{7}$ of their weight of silver.

Native Palladium.

The grains separated from the first solution were, on examination, found to be nearly pure palladium, containing platina, with some traces of iridium. Hence, though the grains of Brasilian platina appear to be free from iridium, yet those of native palladium, which accompany them, afford indications of this substance, and induce a presumption, that, when the mineral shall be obtained in larger quantity, osmium and rhodium may be detected in it.

Dr. W. discovered, that the native palladium in this ore might be distinguished by its external surface exhibiting fibres diverging in some degree from one extremity.

The proportions of the constituents of these ores are not assigned, as the quantities examined were too minute for that purpose.

Observations.—These inquiries are distinguished by the author's usual ability and address. In the examination of minute quantities of bodies, Dr. W. we believe, is inferior to no chemist.

Upon Manganese. By Dr. JOHN.—*Journ. des Mines, No. 130, From the Berlin Journ. de Chim.*

IT might be advisable to use the word *mangane* for the pure metal, and to reserve the word *manganese* for its ores.

The separation of iron from oxide of manganese by succinic acid is very exact, but too expensive for common use. Oxalic acid, or its neutral combinations, may also be employed. Perhaps benzoic acid, which is now become very cheap, might also be used.

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as the benzoate of manganese is soluble. According to Tromsdorff, the benzoate of iron is soluble, but the salt he obtained seems to have contained an excess of acid; in the same manner as an excess of succinic acid produces a similar succinate of iron, crystallizable in brown-yellow tables, which contains a very small proportion of iron.

To use the oxalic acid, a little nitric acid was added to a saturated muriatic solution of manganese, and it was reduced to a syrup, then diluted with water neutralized with potash, and iron added to throw down any copper. When filtered, a little potash was added to begin a precipitation, and oxalate of potash was poured into the turbid liquor, until the iron and lime was entirely separated. If the solution contains lead, it may be precipitated by sulphate of soda. A little prussiate of soda throws down the oxide of manganese of a snow-white, if no copper remains in it; for otherwise the precipitate is peach-red, or scarlet. The white precipitate does not change its colour. A solution of gall-nuts does not cause any precipitation. A very small quantity of a solution of iron being added, causes a blue precipitate to be thrown down by the prussiate.

Copper cannot be entirely separated by the above method, nor could it be got rid of by four successive decompositions of the solution by carbonate of ammonia, and washing of the precipitate with ammonia; for when the liquor is evaporated, the blue colour of copper is rendered visible, and a bluish white triple salt, or carbonate of manganese and copper, is deposited. To produce an entire separation of the copper, the carbonate obtained, by adding carbonate of ammonia to muriate of manganese, and washing the precipitate with water and ammonia, is dissolved in sulphuric acid, and the excess of acid removed by ammonia or carbonate of potash, and on digestion it lets fall a flaky precipitate, containing the copper with a little of the manganese: the transparent liquor being then precipitated by carbonate of ammonia, yields a pure carbonate of manganese, provided the ore does not contain alumine.

As the metal is of very difficult fusion, and the oxide vitrifies immediately if it touches a flux, or comes into contact with the side of the crucible, it has hitherto been impossible to procure it in sufficient quantity, and it is always in grains. To obtain it in an ingot, a small Hessian crucible was lined with a mixture of moist silica and clay, with a deal of charcoal, about half an inch thick on the side, but more at the bottom; powder of charcoal was then passed upon the mixture, and a smooth conical hole made in it; the crucible was then dried, and heated gradually to redness. Carbonate of manganese was ignited for an hour, mixed with oil heated, powdered, and again mixed with oil to form a stiff paste, of a conical form, to fit into the hole in the lined crucible, which is then filled up with charcoal powder, and heated

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previous to the cover being luted on; a very strong heat was then given for an hour and half: 830 grains of the carbonate yielded 250 grains of metal.

Metallic manganese is gray, approaching to silver colour, nearly similar to black cast iron. In the air it acquires a kind of rancid smell. It is not very brilliant. The fractured surface is unequal and fine-grained; it is brittle like cast iron, and easily broken. Its specific gravity is 8.013. It is not magnetic, but a very minute quantity of iron gives it that property. When perfectly pure, it very speedily becomes yellow, then violet colour, and lastly it falls into a dark brown powder. It is also altered in alkohol, but may be preserved in quicksilver; by contact for a long time, a pellicle is formed at the surface of the quicksilver.

The existence of native manganese has been disputed, because the metal is so easily oxidizable; but if manganese contains the minutest portion of iron, it does not fall to powder in the air, and it is magnetic. Pure manganese, in contact with charcoal, attracts oxygen, and falls to powder, so that it is necessary to break the crucible while yet hot. It melts with borax without dissolving in that salt, and then has the appearance of tellurium, in colour, brilliancy, and fineness of grain; it does not alter so soon in the air, nor does it yield any carbonaceous residuum when dissolved in acids. Manganese, reduced in a lined crucible, yields, on solution in acids, a carbonaceous residuum, which, if the manganese also contains iron, is more considerable, and amounts to more than one per cent.

Metallic manganese is reduced, in water saturated by carbonic acid, in a few days, into green oxide, which, after some weeks, changes into carbonate, of which a very small quantity is dissolved in the liquor. The metal pulverized, and placed in carbonic acid gas, changed first into green oxide, and afterwards into a white carbonate.

Clear brown oxide of manganese, formed by the air, is partly changed by carbonic acid into carbonate, and becomes of a brownish gray. Nitric acid disengaged carbonic acid from it, and left the insoluble black oxide.

All solutions of manganese are decomposed by the alkaline carbonates. A snow-white *carbonate* is deposited when the oxidizement is at the minimum; the oxide in this carbonate may be oxidized further by heating it, and a complete oxide is obtained, insoluble in acids. Yellowish or brown carbonate, if otherwise pure, is probably a mixture of brown oxide and pure carbonate. Carbonate of manganese decomposes oxymuriatic acid, and carbonic acid is disengaged. A muriatic solution, containing much oxymuriatic acid, yields, by the alkaline carbonates, a brown or black oxide, which does not contain a trace of carbonic acid.

Pure carbonate of manganese is a smooth white powder, tasteless, and unalterable in the air at 12° R. (60° Fahr.). It keeps

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very well in a close vessel, and may be dried by a heat of 20° R. (77° Fahr.), without alteration. The pure alkalies decompose it, and the white oxide that is separated soon changes brown in the air: 8oz. of water dissolve scarcely half a grain of it. The same quantity of water, saturated with carbonic acid, takes up only about a grain of newly precipitated and yet moist carbonate. The carbonic solution does not yield a pellicle by contact of air. A solution of manganese in an excess of acid being decomposed by an alkaline carbonate, and evaporated, yields a pellicle; but this is composed not only of carbonic acid and manganese, but also of the precipitating alkali, and perhaps of the acid formerly united with the manganese. To decompose a solution of manganese completely, the acid must not be in excess.

Carbonate of manganese is soluble in fat oils, and thickens them. On solution in an acid, it loses 34.25 per cent. of its weight: 120 grains yielded, on distillation, carbonic acid gas, and 12 grains of water, and left 67 grains of a greenish-gray oxide, so that it contains 55.84 per cent. of incomplete oxide, 10 of water, and 34.16 of carbonic acid. The oxide dissolves without effervescence in acids, and is the real basis of salts of manganese, as the white oxide thrown down from them is either a hydrate, or an oxide that retains some of the acid.

Concentrated *sulphuric acid* has scarcely any action upon metallic manganese; but when diluted, the action is more rapid, heat is produced, and hydrogen is disengaged, which has a peculiar smell from the metal it carries off with it. The liquor becomes green, but changes to a clear rose-colour; and, if the solution is diluted, this colour becomes imperceptible: 190 grains of sulphuric acid, sp. grav. 1.860, dissolve 100.75 of metallic manganese, which left 0.75 grains of black, shining, and friable charcoal. The solution, decomposed by carbonate of ammonia, yielded 205.75 grains of carbonate; therefore the carbonate contains 48.6 per cent. of metal, and 51.4 of acid, water, and oxygen. The oxidule of manganese is soluble in sulphuric acid of every strength, and the solution is perfectly similar to that of the metal. The sulphuric solution, briskly evaporated, deposits a granular powder, and yields a pellicle; but it does not crystallize. Crystals are obtained by spontaneous evaporation of a concentrated solution, especially if some already formed crystals are added, and the solution kept in a warm place. The first crystals are pale rose, the last white, and containing an excess of acid.

Sulphate of manganese crystallizes in compressed quadrangular prisms, either complete or truncated on the edges, which are frequently interlaced together; or in rhomboids. The crystals are quite transparent, pale rose-red, of a bitter metallic taste, not alterable in the air at the temperature of 10° R. (55° Fahr.). The crystallized sulphate does not oxidize any further, unless placed in oxygen. At 10° R. one part of it is soluble in two and a half

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of water. It is not soluble in alcohol. The specific gravity is 1 834. At 15° R. (66° Fahr.) it becomes white and opake. When dried by a red heat, it is redissoluble in water; but by a stronger heat, sulphurous acid is disengaged, and also a little sulphuric acid; the residual sulphate is united with black oxide.

The watery solution of the sulphate is not precipitated by oxalate of potash, nor by borate of soda; but common borax throws down a borate of manganese. Tartarous acid, infusion of gall-nuts, the alkaline chromates, or arseniates, do not produce any alteration in the sulphate; but the alkaline prussiates, carbonates, and phosphates, throw down white precipitates, being salts of manganese. The smallest quantity of copper, added to the sulphate, causes the prussiate to acquire a peach-red colour.

150 grains of sulphate, decomposed by muriate of barytes, yielded 148.5 grains of sulphate of barytes previously ignited; carbonate of potash, added to the residual solution, threw down 83.25 grains of carbonate; hence the sulphate contains 33.66 per cent. of sulphuric acid, 31 of suboxide, and 35.34 of water.

Oxymuriatic acid gas, previously passed through caustic potash, separated a brown or black oxide from sulphate of manganese; on applying heat, the decomposition became more rapid, and the liquor contained an excess of acid. By boiling nearly to dryness, the oxide was again taken up, a small quantity only remaining, and reddish crystals were obtained. Alcohol, digested upon the crystalline mass, filtered and evaporated, yields small deliquescent crystals of muriate of manganese, which are decomposable by sulphate of silver. The supernatant liquor was not precipitable by sulphate of silver; but muriate of barytes threw down a precipitate not soluble in acids, so that it contained sulphate of manganese.

An acid solution of sulphate of manganese, neutralized with ammonia, and evaporated, yields rose-colour, rhomboidal, transparent crystals, imbedded one in another, deliquescent, very soluble in water, and emitting ammonia on addition of potash; in other respects they resemble sulphate of manganese.

Concentrated sulphuric acid dissolves saturated oxide of manganese without heat; the solution has always an excess of acid, is dark violet, which grows paler on adding water, and changes to blood-red. By a slow evaporation it is not altered, but by a greater heat it loses its colour, the oxide loses part of its oxygen, and a solution of sulphate of manganese, with excess of acid, remains. Alcohol, added to the original solution, and heated, takes away its colour, without any precipitation, and the alkalies then throw down white precipitates.

The sulphate of the black oxide is precipitated by pure alkalies of a dark brown, by carbonates of a dark brown red, and by prussiates of a yellowish brown. The precipitates become darker by the contact of air. Those by the carbonates do not contain carbonic acid. This sulphate does not form crystals.

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Dilute sulphuric acid, digested upon the black oxide, forms an amethyst-colour solution, containing an excess of acid, not crystallizable, precipitated brownish-red by alkalies.

Three parts of concentrated sulphuric acid, distilled upon two of black oxide, yields oxygen, at a much less heat, and in greater quantity, than that gas could be disengaged from the oxide itself, as also water and sulphuric acid, and leaves a white spongy mass soluble in water, and leaving rose-colour crystals on evaporation; so that, as sulphate of iron is decomposed by a red heat, this method may be used to separate iron from manganese. But by a white heat this sulphate itself is decomposed, sulphurous acid is separated, and black oxide is left. Crystallized sulphuric acid, holding a little manganese, is found in the neck of the retort, which throws some light on the fuming matter in (real) oil of vitriol.

Sulphurous acid, passed from a vessel containing water into water in which carbonate of manganese was suspended, produced a violent effervescence, and threw down white sulphite of manganese. The supernatant liquor was very acid, smelled of burning sulphur, and by evaporation became common sulphate, with excess of acid.

The sulphite of manganese is granular, tasteless, insoluble in water or alcohol, not alterable by the air. Sulphuric, muriatic, and nitric acid, separate the sulphurous acid. Being heated, the sulphurous acid is separated, and brown oxide is left: 50 grains of sulphite, decomposed by sulphuric acid, and precipitated by carbonate of potash, yielded 36 grains of carbonate; so that the sulphite contains 40.2 per cent. of suboxide, and 59.8 of acid and water.

Muriatic acid dissolves metallic manganese, and emits hydrogen, and also heat, unless the acid is very dilute: a little charcoal is always left. The concentrated solution is rose-red, and yields crystals of the same colour. Pure suboxide is quietly dissolved, and the carbonate with effervescence: these solutions are rose-red, and may be crystallized by evaporating a portion to dryness, and adding it to the remaining portion.

The crystallized muriate is in long, thick, four-sided plates, the angles of which are sometimes deeply truncated, and the edges bissed. The specific gravity of these crystals is 1.56. They are rose-red, perfectly transparent, and of a burning salt taste; they soon deliquesce in the air, but may be kept in a close vessel at 20° R. At a warmer temperature they are covered with a white powder, and by a stronger heat melt in their own water which evaporates; the salt is decomposed by a red heat, and leaves a black, shining, and crystalline mass which still retains some acid. Water and alcohol dissolve about their own weight of muriate. Paper dipped in the alcoholic solution burns with a light

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ed flame, and throws out many sparks. This solution yields on evaporation transparent crystals.

Sulphuric acid does not decompose the muriate of manganese. 100 gr. of muriate decomposed by nitrate of silver produce 130 gr. of precipitate; carbonate of ammonia, added to the remaining solution, threw down 69 gr. of carbonate. So that muriate of manganese contains 38.5 per cent. of sub-oxide, 20.04 of acid, and 41.46 of water.

Oxymuriatic acid gas passed for six hours through a solution of 300 grains of muriate of manganese, in 12 oz. of water, threw up a scum, turned it yellow, and reduced it almost to a crystalline mass, which became liquid the next day, depositing long crystals. On filtration these crystals disappeared along with the liquor, which was precipitated of a red colour by alkaline carbonates, brown by pure alkalies, and brown-yellow by prussiate. The solution held free oxymuriatic acid, and that might furnish oxygen to the precipitated oxide. On evaporation, the odour of oxymuriatic acid disappeared, black oxide was deposited, and the liquor became red; re-agents then showed that it contained disengaged muriatic acid, and the alkaline carbonates threw down a white precipitate. The evaporation being continued, oxymuriatic acid again appeared, the precipitate was re-dissolved, and the original quantity of muriate of manganese was obtained.

On several Minerals recently analyzed by Mr. Klaproth. By M. TONNELIER.—*Journ. des Mines, No.* 130.

CINNAMON-STONE, according to Klaproth, contains 38.8 per cent. of silica, 31.25 of lime, 21.2 of alumine, and 6.5 of oxide of iron, 2.25 being lost. Werner arranges it with zircon and hyacinth, but the contents are nearly similar to those of idocrase (vesuvian), and also approach to those of prehnite, zoisite, and axinite (Thumerstein): its specific gravity is also only about 3.6. Hauy, from a slight inspection, is inclined to follow Werner's opinion. Mohs, from a careful examination of all its characters, concludes that cinnamon-stone should not be united with zircon, but that it should be placed in the garnet family.

Labrador hornblende, or hyperstene, contains 54.15 per cent. of silica, 14 of magnesia, 2.25 of alumine, 1.5 of lime, 24.5 of oxide of iron, and 1 of water, with some trace of oxide of manganese, 2.6 being lost.

Capillary sulphuretted iron, or haarkies, contains, according to Klaproth, nickel, and a little cobalt and arsenic. Lelievre has observed, that cold nitric acid has no action on this mineral: the acid being evaporated from it, without ebullition, left a slight yellowish tinge on the glass. It is soluble in sulphuric acid. This solution does not render water turbid, so that it does not contain

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bismuth; the residual pyrites is not altered. It does not yield by the blow pipe any smell of arsenic, or of sulphurous acid, but turns black, and is unfusible. It colours glass of borax of a light blue.

Observations.—Lampadius thought he had found in cinnamon-stone, zircon and potash; neither of which have been discovered by Klaproth.

On the Manufacture of Salt.—Journ. des Mines, No. 129.

The salt-works at Moutiers have four graduation-houses with faggots in the usual manner, and a fifth with rope; which method was invented in 1788, by the Chevalier Dubuet. These buildings are erected in a narrow pass into a valley.

The rope graduation-house is built in the form of a crescent, in order to favour the action of the wind; it is 90 metres (yards) long, of which only 70 are occupied by the ropes. The top of the building contains 259 wooden channels 0.13 met. (5 inches) wide, and having the same distance between them. Each of these channels is furnished with 23 endless ropes 0.007 to 0.008 met. (1-3d inch) diameter, which pass through holes made in the channels, and round pulleys fixed to the floor of the building. As each of these endless ropes is 8.28 met. long, the building took upwards of 100,000 met. (60 miles) of rope when it was originally constructed. The salt water is raised to the top of the building by an endless chain of buckets.

The salt water is first passed twice through two common graduating-houses with faggots, and then the two portions being united together, it is passed seven times, or even oftener, through the third, from whence it passes to the fourth. The remainder of the process differs according to the weather.

During the fine season, which lasts three or four months, it is passed from the fourth house to the boilers, and from thence when it boils to the rope graduating-house, where it is repeatedly raised until the greater part of the salt is deposited upon the ropes, which in very fine weather takes 12 or 16 hours. In general, 27 boilings take 45 days. The cords are then coated with common salt, so as to be sometimes 0.06 met. (2 1-3d in.) in diameter: this salt is got off by a cross piece of wood armed with iron plates being raised to the top of the building, and the cords pulled alternately, so as to strike the salt against the plates.

In the bad season, the water is slowly evaporated in the boilers, of which there are four, all of the same size; they are from between 7 and 8 metres long, 5 and 6 broad, and 0.5 met. (19.6 in.) deep. No bullock's blood, nor white of eggs, is added in the boiling, and the use of the hydrometer is left off.

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Observations.—This is only one part of a paper on the salt-works at Moutiers, in the department of Mont-Blanc, by M. Berthier, the other parts being more of a local nature. In another paper, in the Journ. des Mines, No. 131, mention is made of another method of graduation, invented by M. Baader about 4 years ago, and called graduating-tables, which are floorings of carpentry about 70 feet long, and affording a surface of 90,000 square feet, upon which the air acts freely. M. Baader has announced, that he means to give the public a description of his graduating-tables, and of an instrument that he calls a hydrometrograph, by which the quantity of water evaporated in a given time by the several methods of graduation, may be estimated with precision.

On the Oxides of Iron. By Mr. C. F. Bucholz.—*Journ. des Mines, No.* 121; *and Ann. de Chim. Vol.* 65, *from Gehlen's Journ. Apr.* 1807.

Some chemists have stated black oxide of iron to contain from 25 to 27 per cent. of oxygen, and the red from 40 to 49; others assert, that 100 of iron can absorb from 27 to 40 of oxygen. The following experiments were made to determine the point.

100 gr. of filings of iron dissolved in nitric acid, evaporated to dryness and ignited for a quarter of an hour, acquired 42 gr. The same augmentation took place in two other experiments, so that red oxide of iron contains 70.5 of metallic iron, and 29.5 of oxygen. Lavoisier, or his followers, probably confounded the augmentation of 40 on 100 of metal with 40 per cent. of oxygen in the oxide. As to Proust's statement of 48 of oxygen with 100 of iron, he probably considered the precipitate from sulphuric acid by alkalies as pure oxide, whereas it retains some of the acid even after being exposed to a red heat.

The peculiar character of the black or incomplete oxide of iron is to dissolve without effervescence in dilute muriatic acid, and for the solution not to yield a red precipitate by caustic ammonia. It was by these tests that the oxides obtained were examined.

Iron calcined in the open air, at a high temperature, yielded a mixture of red and black oxide.

Iron oxidized by means of water gave a similar result.

Iron treated with very dilute nitric acid sometimes yielded black oxide; but the quantity could not be determined, because the acid disturbed some part of it; if the evaporation was pushed too far, red oxide was formed.

Red oxide calcined with carbonate of ammonia was not changed; when calcined with tartarous acid it did not lose any weight, on account perhaps of the charcoal that was mixed with the product. When calcined with wax it lost some of its weight, but still con-

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tained some red oxide. Calcination with lampblack reduced some of the iron to the metallic state. No dependance can, therefore, be placed upon the proportion of iron which has been determined by this imperfect reduction, as the result is a mixture of iron, and the black and red oxides of that metal. The calcination of succinate of iron was equally unsuccessful.

It was, therefore, necessary to procure black oxide, and convert it into the red kind. The black oxide was procured by plunging ignited iron bars into water, as it was found impossible to obtain it by calcining metallic iron with red oxide in various proportions.

100 gr. of this black oxide dissolved in nitric acid without emitting any nitrous gas; the solution evaporated and calcined left 110 of red oxide, so that the black oxide contains 77.46 of metal, and 22.54 of oxygen, and red oxide contains 90.9 of black oxide and 9.1 of oxygen.

By passing water through an ignited tube, it was found, in a direct manner, that black oxide contains 23.3 of oxygen; but this experiment cannot be depended upon, as it is always uncertain whether all the oxide is detached.

The phenomena which some chemists have attributed to intermediate oxides depend upon the mixture of these two oxides, or on their combination with acids.

Red oxide of iron exposed to a moderate red heat, did not lose any of its weight, its bulk was diminished, and it acquired a metallic appearance, but it yielded a red powder, and was not changed. In two other experiments the oxide was exposed to a white heat for a long time, and the weight of the oxide was diminished, in the last as far as 6.5 per cent. In one the oxide became blackish steel gray, with metallic lustre, and was attractible: in the third, the inferior part of the oxide was entirely melted, the fracture of the mass was smooth steel gray, with a metallic lustre; the upper part was imperfectly melted, crystallized on its surface, and porous in the middle. They were mixtures of the red and black oxides.

There is reason to believe that only the black oxide combines with earths, and that their attraction assists the conversion of red oxide into black, at a white heat.

Black oxide requires boiling in strong nitric acid in order to be dissolved. Red oxide also requires a good quantity of boiling nitric acid of middling strength: diluted acid has no action upon it; but when newly precipitated by potash from an acid, it is soon taken up. The nitric solution of red oxide ought to be looked upon as a solution of nitrate in uncombined acid. The separation of this uncombined acid by iron or evaporation throws down a red nitrate, which is not soluble in water, but is dissolved by fresh acid, and which is commonly taken for a pure oxide; it emits red vapours by calcination, and leaves red oxide. Dilute nitric acid

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forms with iron a nitrate of black oxide, but the oxide soon acquires oxygen, and it is converted into a nitrate of red oxide.

Nitric acid added to a solution of 1 oz. of green sulphate of iron in an oz. of water while boiling, threw down 42 gr., of brownish-yellow neutral sulphate of iron, with excess of oxide. The supernatant brownish-red liquor being evaporated, let fall a brownish substance, which was again taken up as the evaporation proceeded. Sulphuric acid was then added, and heated till the excess of acid was dissipated; a white tasteless powder remained, insoluble in water, which, however, abstracted from it some sulphuric acid, and changed it brown. Ammonia also separated some of the same acid, and changed it to brownish-yellow. Muriatic acid, and very dilute sulphuric acid, dissolved it entirely into a yellowish-red liquor.

When a green solution of perfectly pure iron is precipitated by the alkalies, and especially by ammonia, there is developed a very strong smell of phosphuretted hydrogen, the cause of which is at present unknown.

Observations.—We are by no means of opinion that these experiments are sufficient to decide the dispute respecting the number of oxides formed from iron. The crystallographic researches of Hauy and Bournon show that there are at least four species of oxides of this metal, of which three are crystallizable, and the fourth not so.

The indexes to our volumes will show the number of essays that have been written on this subject, and a review of them will enable our readers to form some opinion.

On Wernerite. By Dr. John.—*Journ. des Mines, No.* 131; *from Gehlen's Journ. June* 1807.

Crystallized white wernerite neither puffs up, nor melts when exposed to the blowpipe on charcoal; neither does it melt with borax; but it forms a globule with microcosmic salt, yellow while hot, but losing its colour when cooled. When strongly heated in a porcelain crucible, it changed to ash-gray, but did not melt.

Melted with five times its weight of caustic potash in a silver crucible, it formed a blue mass, which gave the same colour to water. Muriatic acid dissolved this mass, and formed a reddish-yellow solution, from which oxalate of potash threw down oxalate of lime, the prussiate a blue precipitate, and liquid potash a brown precipitate, that is, for the most part, resoluble in an excess of the alkali.

This variety of wernerite was found to contain 51.5 of silica,

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38 of alumine, 10.45 of lime, 3.5 of oxide of iron, and 1.45 of oxide of manganese and lost matters.

Green wernerite treated by the blowpipe on charcoal puffed up a little on the sharp edges, and changed from pistachio or olive green to brown green. Borax, or microcosmic salt, dissolves a small quantity, and forms a dark yellowish green globule, which loses its colour on cooling. Heated in a porcelain crucible, it lost 2.85 per cent. of its weight, changed to a chocolate colour, and became quite opake, but without any sign of fusion.

Pulverized wernerite yields a gelatinous deposit of silica with nitric or muriatic acid; the solution itself is yellow.

This variety contained 40 per cent. of silica, 34 of alumine, 16 of lime, 8 of oxide of iron, and 1.5 of oxide of manganese.

Observations.—Monteiro has since published, in the Journ. de Physique for February 1809, a paper on the analogy of wernerite with paranthine (scapolite), an abstract of which will be given hereafter.

Analysis of some Iron Ores, and Products of the smelting Furnace. By M. GUENYVEAU.—*Journ. des Mines, No.* 132.

THE first essay of smelting iron with coak, that was made in France, was tried at Creusot; but the ore was very bad, and of course the trial was unsuccessful.

The coak made from coal of the principal bed, which is caking and extracted in large masses, was found to contain 96.7 per cent. of charcoal, 3 of earthy residuum, and 0.3 of sulphur; so that it is a most excellent fuel.

The coak made from the small coals of the galleries, which leaves a considerable residuum when burned, and is, when prepared, difficult to burn, and not used by itself for smelting, was found to contain 89.24 per cent. of charcoal, 6.66 of oxide of iron, 3 of lime, 0.77 of silica, and 0.33 of alumine.

Three kinds of ores are smelted at Creusot: 1. Chalancey ore. 2. Riminy ore, called cold ore, which is mixed with the former. Both these are smelted in a furnace, 13 metres (yards) in height, without any addition of other matters, and the iron that comes from them has not sufficient tenacity to form cannon, but is used for ballast. 3. Franche Comté ore, in grains, which is smelted with charcoal in a furnace 7.13 met. high, and yields excellent iron.

The Chalancey ore is found in small agglutinated grains, in layers between beds of limestone. It yields with glass of borax, 15 per cent. of iron, but in the smelting furnace nearly 25. It was found to contain 31 per cent. of lime, 30 of water and carbonic acid, 29 of red oxide of iron, 6.5 of silica,

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1 of alumine, 0.21 of phosphorus, no manganese nor chrome, but 3.29 were lost.

The Rimmy ore is compact and hard; it is considered as very poor, but yielding good iron: it is only used to assist the fusion of the other ore, and to reinstate the walls of the furnace, when corroded. It was found to contain 68.6 per cent. of silica, 24.6 of red oxide of iron, 7 of water and carbonic acid, 1 of alumine, some traces of lime and manganese, and 0.8 were lost.

When the fusion goes on well, the scoriæ that flow out are white, or grayish, commonly opake, and contain 49.6 per cent. of silica, 30 of lime, 15 of alumine, 3 of oxide of iron, without any manganese or chrome. It is probable that this is always the composition of scoriæ from iron smelting furnaces, when the operation is properly performed; for the scoriæ from Geislautern, where argillaceous ore of iron is smelted by the addition of lime, being examined, were found to contain 49 per cent. of silica, 30 of lime, 15 of alumine, 3 of red oxide of iron, 1 of oxide of manganese, and some traces of copper. The coincidence of these analyses is remarkable.

The scoriæ that run when the fusion does not go on well, are very porous, almost black, and do not contain any globules of iron. They contain 39.5 per cent. of silica, 35.6 of lime, 18 of alumine, 3 of red oxide of iron, and some traces of manganese. So that the silica is in much less proportion, and the other earths in a larger than in the former scoriæ.

The art of smelting iron in high furnaces may be reduced to these few points: To find the proper admixture of ores and other materials, which will yield scoriæ, at the temperature of the furnace, that have sufficient fluidity and will not corrode the sides of the fire-place: to keep the temperature at a standard height: and to know how to remedy the unforeseen variations that may occur in the constituent ingredients of the ores and fuel.

The iron run from this furnace is gray, very fine grained, and rather dull; it is very brittle, and may be reduced to powder, but is soft to the file, and fills the moulds well. It was found to contain 93.15 per cent. of metallic iron, 3.5 of silica, 2.1 of charcoal, 0.8 of alumine, 0.75 of phosphorus, 0.5 of lime, and 0.3 of sulphur, with some trace of manganese, but none of chrome. The brittleness probably depends upon the phosphorus, and does not arise from its being smelted with coak; for the Franche Comté ore being smelted with coak yielded, as with charcoal, a very good iron proper for cannons.

A specimen of iron, sent to the Board of Mines, as being of very good quality for cannons, being analyzed, yielded 96.79 of metallic iron, 2.4 of charcoal, 0.54 of silica, 0.27 of phosphorus, a little chrome, a trace of alumine, but no sulphur.

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Observations.—This paper may be said to contain the elements of the art of smelting iron ores. The theory of smelting cannot be improved in any other way, than by that now in progress in the mining schools of France, of analyzing the ores, additions and fuel employed in the smelting, and again observing, by counter-analyses, the new disposition of the former principles among the scoriæ and smelted metals. The great similarity between the scoriæ arising from two ores of different compositions, is very striking, and shows that, by attending to the analysis of the scoriæ proceeding from the smelting of an ore, a judgment may be formed of the addition necessary to be made to the ore in order to perfect the fusion.

On Bronzite. By Mr. KLAPROTH.—*Journ. des Mines, No.* 132, *and Ann. de Chim. Vol. LXV. from Gehlen's Journ. June* 1807.

BRONZITE is a pale bronze-coloured stone, found in masses or large lumps, of a brilliant and metallic appearance; it is lamellar, and the flakes are very distinct in one direction: the thinner flakes are very transparent, although it is opake in the lump. The scrapings of it are white. It is not very hard, but very brittle. The specific gravity is 3.2.

It becomes paler, and lost one half per cent. of its weight by exposure for half an hour to a red heat.

It was found to contain 60 per cent. of silica, 27.5 of magnesia, and 10.5 of oxide of iron, 0.5 being lost, as above stated.

It is usually found in small pieces, disseminated in serpentine.

Schillerstein, schillerspath, or schillerblende, which is also found in serpentine, is said by Heyer to contain 23.3 per cent. of alumine, and by Gmelin 18. So that, if these analyses are right, it must be united with shotten hornblende, as has been done by Karsten.

Hauy has united smaragdite and bronzite into one species, under the name of diallage, from which he has separated Labrador hornblende, and made that a peculiar species under the name of hyperstène: but Vauquelin has found in smaragdite a notable quantity of lime, and only a small proportion of magnesia. Bronzite also is not fusible by the blowpipe, whereas smaragdite yields scoriæ.

Observations.—Great confusion has arisen in the classification and determination of these metallic-looking stones; but this confusion seems approaching to its end, as it appears evident that there are at least three separate species of these stones.

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On phosphoric Acid, and the Phosphates of Lime, Iron, Lead, and Manganese. By M. Berthier.—*Journ. des Mines, No.* 132.

The investigation of these substances is important, because they are combined with the most common ores of iron, and unite with the smelted metal and scoriæ.

Phosphorus was acidified by nitric acid, and the liquor being saturated with ammonia, was employed to prepare the phosphates by double decomposition.

Seven grains of arragonite dissolved in muriatic acid, yielded by addition of the phosphate of ammonia, and a slight excess of ammonia, 5.5 of calcined phosphate; by driving off this excess, 0.3 more of phosphate was deposited. The remainder of the lime was separated by oxalate of ammonia, the precipitate calcined left 0.78 of quick lime. As arragonite contains 56 per cent. of lime, phosphate of lime contains 54 per cent. of lime, and 46 of acid.

Phosphate of lime is white, gelatinous, and voluminous; it strongly retains water even to a red heat; it requires a very high temperature to melt it, and is irreducible. Although calcined it is soluble in nitric, muriatic, or sulphuric acid. Acetous acid readily dissolves it when moist, but with difficulty when calcined. It is precipitated from these solutions by hydrosulphuret of ammonia, or by ammonia, but is redissolved by an excess of alkali. It is also precipitated by the alkaline carbonates, but is redissolved if they are added to excess: in the dry way they decompose the phosphate with difficulty. It is partly decomposed by pure potash, either in solution, or in fusion; in the latter case there remains, after washing with water, a phosphate with excess of lime, which may be decomposed by acetous acid.

Five grains of red oxide of iron, dissolved in muriatic acid, and precipitated by phosphate of ammonia, let fall a phosphate of iron ad maximum, which being calcined, weighed 9.6; so that it contained 52 per cent. of red oxide of iron, and 48 of acid.

This phosphate was gelatinous, yellowish, white, and bulky: it retains 8 or 9 per cent. of water, until it is heated to redness, and ready to melt; when melted, it becomes brown. It is easily reduced in a lined crucible, and yields 45 per cent. of phosphuret of iron, which is more easily melted than cast iron, is lamellar, whitish-gray, and pulverisable. It is not soluble in water or acetous acid, but dissolves in the strong acids, and is precipitated from them without alteration by carbonate of potash. It is decomposed by the fixed alkalies at a boiling heat. Concentrated ammonia re-dissolves it, and forms a red liquor, which deposits the oxide on boiling: carbonate of ammonia produces the same effect. Hydrosulphurets and prussiates decompose the solutions of

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this phosphate, sulphurous acid and sulphuretted hydrogen reduce it ad minimum. There is no way to separate the excess of oxide from the phosphate: the reduction of both ad minimum, by sulphurous acid, and subsequent saturation with metallic iron, did not succeed.

An alkaline phosphate throws down from a solution of iron ad minimum, a white bulky precipitate, which first changes to gray, and afterwards to a greenish blue. This colour is destroyed by heat, and is changed to a deep ochre-yellow phosphate of iron ad maximum, with excess of oxide.

The phosphate ad minimum is very easily altered; when pure it is white, and is thrown down of the same colour by prussiates. It dissolves in acetous acid, the solution deposits yellowish phosphate ad maximum, by boiling. Nitric acid dissolves it, disengaging nitrous gas, and changing it ad maximum. Other acids dissolve it without altering it. Saturated carbonates precipitate it from these solutions. Ammonia, and carbonate of ammonia, redissolve it of a brown-yellow. Fixed alkalies, prussiates, and hydrosulphurets decompose it. When blue, it already contains phosphate ad maximum, and produces a blue prussiate, which is still deeper when the phosphate is ochre-yellow. Its composition is not ascertained, but it does not contain so much acid.

Ten grains of phosphate of lead, obtained by adding phosphate of ammonia to nitrate of lead, being dissolved in nitric acid, and precipitated by sulphuric acid, yielded 10.45 of sulphate of lead, which contains 74 per cent. of oxide of lead; so that the phosphate contains, after correcting this experiment by others, 77.5 per cent. of oxide and 22.5 of acid.

Phosphate of lead is white, flaky, but heavy: it may be heated to redness without melting. Being ground with oil, and melted in a lined crucible, it was partly reduced; the scoriæ were white, well melted, opake, crystalline, and in large laminæ. The lead was very soft and pure. The phosphate is soluble in nitric acid, and very difficultly in acetous acid: muriatic and sulphuric acid decompose it completely. Alkalies and carbonates precipitate it, and, if in excess, partly decompose it; if the excess is considerable, they, especially potash, retain a little oxide. It is not soluble in ammonia; prussiates and hydrosulphurets decompose it completely.

Hydrosulphuret of ammonia was poured into a solution of 5.25 gr. of phosphate of manganese in muriatic acid; the supernatant acid yielded 4.8 of phosphate of lime; 0 3 more were afterwards separated from the precipitated hydrosulphuret of manganese: so that phosphate of manganese contains 44.4 per cent. of acid, and 55.6 of oxide ad minimum.

Phosphate of manganese is as white and bulky as that of lime; it may be ignited without melting; its colour tarnishes. It is soluble in all acids, even the acetous, if still moist. Fixed alkalies precipitate it from these solutions, and, if added to excess, decompose

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it. Prussiates and hydrosulphurets decompose it. Ammonia redissolves it; the solution is colourless, and lets fall all the oxide, of a brown colour, on boiling. 5.1 gr. mixed with oil, and melted, yielded, even on repeating the fusion, only 0.1 of very brittle metal: the scoriæ were well melted, compact and green, and weighed 3.9; so that this phosphate, with excess of oxide, contained 29.3 per cent. of acid, and 70.7 of oxide. The easy fusibility and difficult reduction of this phosphate, point out the use of certain oxides of manganese in purifying the phosphuretted kinds of iron.

One grain of phosphorus was acidified by nitric acid, saturated with ammonia; this phosphate threw down 4.8 of precipitate from muriate of lime; hence the phosphate of lime contains 20.9 per cent. of phosphorus, and the phosphoric acid consists of 45.4 per cent. of phosphorus, and 54.6 of oxygen.

1.2 gr. of phosphorus acidified by nitric acid, was mixed with a solution of 2.5 of red oxide of iron; and neutralized with ammonia, it yielded 4.8 of phosphate of iron: the liquor did not retain any iron, but formed with muriate of lime 0.56 of phosphate: hence 46.9 of phosphorus produced 100 of acid.

Nitrate of lead holding 13 gr. of metal, and 1.5 of phosphorus reduced into phosphoric acid, yielded 16.95 of phosphate of lead; the remaining acid produced 0.6 phosphate of lime: as oxide of lead contains 7 per cent. of oxygen, 46.1 of phosphorus produced 100 of acid. The phosphate contained an excess of oxide, for it was composed of 17.58 per cent. of acid, and 82.42 of oxide.

Muriate of manganese precipitated by phosphate of ammonia, formed from 1.3 gr. of phosphorus, let fall at least 5.1 of phosphate of manganese, and there remained as much acid as formed 1.15 of phosphate of lime. Admitting 46.1 per cent. of phosphorus in the acid, the former statement of the composition of phosphate of manganese is confirmed.

It further appears that the whole of the phosphorus contained in phosphate of iron, does not enter into the phosphuret formed from it. 100 of phosphate produce 45 of phosphuret, so that allowing the red oxide of iron contains 30 per cent. of oxygen, 13.6 of phosphorus, equal to 29.5 of acid, is lost; but if the phosphate acquires an excess of base before it is reduced, it ought to contain 73.7 per cent. of oxide, and 26.3 of acid.

Phosphuret of iron contains 20 per cent. of phosphorus, and 80 of iron.

Phosphoric acid is colourless, perfectly transparent, and ductile like horn, unless too long exposed to heat. 10 gr. of solid acid, prepared in the old method from bones, produced 12.5 of phosphate of lime, equal to 5.75 of dry acid; it also contained several soluble phosphates. It is, however, impossible to separate the water from the acid by heat, and the acid itself is volatile at a heat below ignition. Heated to redness for a few minutes in a platina

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crucible, it retained 40.2 per cent. of water, so that it only contained 59.8 per cent. of acid, equally dry as in calcined phosphate of lime.

Observations.—As the ores of iron, which are most usually smelted, contain phosphorus, its acid, and manganese, any addition to our knowledge of those substances must be highly useful; and the more so, when experiments are made with so much care, and related so clearly, as the present. The papers furnished by M. Berthier rank among the best in the Journal des Mines.

On red Zeolite of the Tyrol. By M. LAUGIER.—*Journ. de Phys. December* 1807.

FAUJAS found the red zeolite in the valley of Zuicanti, in the Vicentine.

It is radiated like zeolite, and appears to be composed of small compressed prisms: it does not form a jelly with acids: It contains about 1-6th of calcareous carbonate, which was separated by means of nitric acid.

On analysis, it yielded 45 per cent. of silica, 16 of uncombined carbonate of lime, 11 of combined lime, 10 of alumine, 4 of oxidized iron, 0.5 of oxidized manganese, 12 of water, and 1.5 were lost.

It resembles tremolite (or grammatite) by its radiated needles, and by the rhomboidal plates of the limestone which is intimately mixed with it; while in other respects it resembles stilbite.

When two bodies are composed of the same principles, in different proportions, it is difficult to determine precisely their respective limits; but if they contain other principles, they can be easily and truly separated by means of their analysis. Now tremolite and stilbite are very distinct in their composition. Tremolite contains much magnesia, which probably comes from the dolomite in which it is found, and it does not contain any alumine. Stilbite, on the other hand, abounds in alumine, and contains no magnesia: hence it is easily distinguished from tremolite; and red zeolite of the Tyrol is evidently a stilbite.

On bleaching Silk. By M. ROAD.—*Ann. de Chim. Vol. LXV.*

In the former part of this memoir, the action of light, water, and other agents, upon raw silk, is examined, and also the substances found united with it; in the latter, the principles deducible from that examination are applied to the bleaching of silk.

White raw silk becomes still whiter by the sun; the fine yellow silk entirely loses its colour, and becomes of a good white; the dirty yellow silk acquires, by a long exposure to the light, a

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dingy reddish white colour. Yellow silk, partly bleached by soap, being exposed for four or five days to the sun, is rendered of many shades of white.

Yellow raw silk, boiled for eight hours in water, and the water evaporated, left, 1. a dry, friable, reddish, resin-like substance, very soluble in water; 2. colouring matter, very soluble in alcohol, forming a greenish-yellow solution; 3. a substance insoluble in boiling water, soluble in hot alcohol, and falling down as it cools in light and very white flakes; 4. a blackish-brown residue, burning like animal substances, insoluble in water or alcohol, but soluble in concentrated acids. White raw silk yields the same products, except the colouring matter.

Yellow raw silk was boiled for some hours in alcohol, and the solution poured off while hot. The solution was very clear, and of a fine gold colour, more or less deep; on cooling, a flaky substance fell down, and remained upon the filter in the form of a fine, yellow, opake jelly, which was very much reduced by drying, and formed only very light and thin pellicles. The colouring matter was separated from it by cold alcohol. The fat matter, insoluble in cold alcohol, was analogous to wax, spermaceti, and adipocire. Fine yellow silk becomes, by this operation, of a good white, but ordinary silk remains coloured. This reddish tinge may be given to yellow raw silk bleached by the sun, by leaving it for some months on the grass, macerating it in oxymuriatic acid, or by boiling it for some hours in alcohol.

Although oxymuriatic acid does not change the colour of yellow raw silk, it produces a remarkable change in the gummy substance. Solutions of caustic soda, or of neutralized soda, boiled for several hours on silk, did not take up so much soluble matter as alcohol.

The good effects of soap have been long known; but it was thought prudent to analyze that made use of, as also the water. The soap was found to contain 52.29 per cent. of water, 41.58 of oil, and 6.13 of soda: but, according to Lelievre, Darcet, and Pelletier, the soap they examined contained only 30.46 per cent. of water, 60.95 of oil, and 8.59 of soda; so that soap may contain more than 20 per cent. of superfluous water, without losing its solid form.

The Seine water, at Paris, yielded, on evaporating, 10 litres (610.3 cub. in.) of it, on an average 2 grammes (30.8 grains) of muriate of lime mixed with carbonate, equal to 1.21 grammes of lime. From this it appears, that in bleaching 30 chilogrammes (about 60 lb.) of silk, with 7.5 (about 15 lb.) of soap and 750 (about 1500 lb.) of water, 0.6 (about 1 lb. 5 oz.) of soap is decomposed; and when water from the Gobelins river is used, which contains more calcareous salts, 1.8, or about a quarter of the soap, is rendered useless.

Raw silk, treated with a solution of soap, yielded as much gum,

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and more waxy and colouring matter, than when either water or alkalies were employed: and on examining, by means of alcohol, silk that had been treated with different liquids, that which had been treated with a solution of soap yielded very little fat or colouring matter to the alcohol, and much less than the other specimens.

The stiffness of silk is owing to gum, which forms 23 or 24 per cent. of it. When dry, the gum is friable, and yields a very light reddish-yellow powder, smelling and tasting like extracts. It is not softened by heat, but blackens, and is converted into coal. It is soluble in six times its weight of water, forming a transparent brown-red solution, which is yellow when spread out thin, and becomes greenish and putrid when exposed to the air. The solution lathers like that of soap. Concentrated sulphuric or muriatic acid deepens the colour of this solution; nitric acid changes it to a golden yellow; sulphurous acid, and especially the gas, brightens the solution, and changes it to a pale greenish yellow. Alcohol of the specific gravity of 0.8293, and at 80° Cels. (176° Fahr.), does not dissolve this gum. Oxymuriatic acid throws down, from a solution of the gum, an abundant white precipitate, which becomes reddish in the air; its solubility in water is not altered, but it is rendered soluble in alcohol, whether hot or cold. Alkalies do not alter the solution of the gum, but nut-galls throw down a precipitate. As Lyons could furnish several hundred tons of this substance, if it were separated from the soap, it might be of utility.

The colouring matter, which is only found in yellow silk, is a resinous substance, almost solid at 12° Cels. (54° Fahr.), and entirely liquid at 30° (86° F.), which forms about 1-55th or 1-60th per cent. of the silk. It has a strong smell, arising from the volatile oil combined with it, which resembles oil of aniseed, and may be separated from the yellow raw silk by means of alcohol. The most concentrated solutions of this colouring matter are rendered colourless in a few days by the sun. It is not soluble in water, but soluble in 8 or 10 times its weight of alcohol. Caustic alkalies, especially ammonia, has some action upon it. The solution of soap, although it has but little action in the cold, dissolves a greater quantity than the alkalies at a boiling heat. Sulphuric and muriatic acids render it black, but sulphurous acid partly takes away its colour. Oxymuriatic acid converts it immediately into a solid white substance analogous to wax.

The wax of silk is found in all kinds of silk, and forms about 1-200th or 1-300th part of China silk. It is hard, brittle, and slightly coloured; it melts at 75 or 80° (167 or 176° Fahr.). Insoluble in water, but very soluble in alkalies or soap. Alcohol, sp. grav. 0.8293, takes up only 1-2000th of its weight at 20 or 25° (68 or 77° Fahr.); and, at a boiling heat, about 1-300th or 1-400th. The solution is scarcely coloured, it grows turbid even

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while hot, and lets fall white flakes. When cooled, these form a bluish white mass, which diminishes greatly on drying, and is reduced to very thin plates, slightly transparent.

White raw silk contains only this wax, and some oil, which with gum appears to give colour to silk.

Silk is usually treated with a solution of soap, in order to render it soft, white, and brilliant, without affecting its strength.

White raw silk, boiled for three hours with 300 parts of water, is rendered soft and brilliant; but yellow requires 400 parts, and four or five hours boiling, and even then it retains much of its colour. It requires 1-100th part of pure caustic soda, with the usual quantity of water, and usual time of boiling, as when soap is employed; to prepare white raw silk; and 1-75th or 1-80th, to prepare yellow raw silk. The preparation with soap varies in different manufactories; but, on the average, yellow raw silk to be bleached requires 60 or 70 chiliogrammes of soap for 100 of silk, and at least four or six hours boiling; the white raw silk only 30 or 35 of soap, and four hours boiling: the mean proportion of water being 25 or 30 times the weight of the silk.

Comparative experiments being made, the white raw silk, prepared with soap, was very white and brilliant; with soda soft, but less white, as having a yellowish-gray tinge; with water, although very soft, dull, and having a yellowish-gray tinge. Yellow raw silk exhibited the same differences, but the soda had not acted so strongly upon it as the water. The white silks lost upon an average 24 to 25 per cent. and the yellow 26 to 28. The silks prepared with soap were stronger than those boiled in water, or with soda; and were of superior brilliancy when dried.

In boiling silk, copper vessels have some inconveniencies, on account of the ease with which they are oxidized: and as the calcareous salts present in water diminish the soap, it is necessary to use very pure water, and only a certain quantity, which is 15 or 16 times the weight of the silk: 1-12th or 1-6th of soap is sufficient for white raw silk; but for yellow raw silk it is necessary to add 50 or 60 per cent. of soap. Even an equal weight of soap, and the subsequent use of sulphurous acid gas, does not render these silks as white as the other, when treated with 25 per cent. of soap. As to the time of boiling, the silk which was boiled the least time was whiter, more brilliant, and had lost less of its weight than that which had boiled a longer time.

Silk, if the boiling be continued too long, loses the white colour it has acquired; the following experiments were made to determine the cause of this alteration: White raw silk, boiled in a vessel which permitted 2-5ths of the water to be evaporated, was not so white as that boiled in a vessel that did not permit any evaporation to take place.

Very white silk, which had been boiled with soap, was again boiled for an hour in the soap-liquor and in solution of gum; it

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acquired a reddish tint, that could not be got rid of by boiling water.

Silk, already bleached by soap, was boiled again for four hours with a quarter of its weight of soap. The white raw silk, thus doubly bleached, had a greenish-gray tint; it was dull and harsh, having acquired some resemblance to thread in hardness; it had lost 1-53d of its weight, and 7 per cent. of its strength. The yellow raw silk was whiter than at the first bleaching, but had lost some of its softness and brilliancy, with 1-107th of its weight, and 5 or 6 per cent. of its strength.

White raw silk, bleached by soap, and very white, being boiled, after careful washing, for many hours in distilled water, and the water afterwards evaporated, yielded a small quantity of animal matter, not analogous to the products above spoken of, but which burned in the same manner as silk.

As silk is completely bleached in less than an hour, the boiling should not exceed that time; the soap and silk being put about half an hour before the water boils, and the latter frequently turned. A less time would suffice for trames and organzines.

It is to the alterations which take place when the boiling is too long continued, that the impossibility of aluming silk in the hot-bath is owing; and the loss of brilliancy when silk is dyed of colours rather brown, for which a boiling heat is necessary.

Observations.—These experiments seem to be of importance to the manufacturers and dyers of silk. It were to be wished that we possessed a set of experiments upon a plan equally regular in respect to the preparation of our staple commodity of wool.

On an Apparatus for moulding Phosphorus into Sticks. By M. Destouches.—*Annales de Chim. Vol. LXV.*

The old methods of moulding phosphorus into sticks being imperfect or dangerous, the following apparatus was invented, composed of two parts:

The first is a small inverted brass cone, with a handle fastened to one side, and a brass cock at the lower or narrow end; this cock terminates in a small tube, that can be inserted into the mouths of the moulding tubes, which are stopped, at their lower end, with cork or fat lute. These tubes are retained, in great number and in an upright position, upon a brass dish an inch deep, by two diaphragms pierced with holes, and kept at a convenient distance from each other, and from the dish, by a middle stem, which has a ring at the end, so that the whole of the tubes may be lifted up at once by means of a hook.

The dish, with the tubes in it, is plunged into warm water, so that the orifices of the tubes, which are to be previously filled

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with water, may be about two or three millimetres (1-10th of an inch) above the surface. Some phosphorus is put into the conical vessel, and melted by means of boiling water; the end of the cock being then put into the orifice of a tube, it is turned, and the tube filled with phosphorus to 5-6ths of its height. When all the tubes are filled, the dish is lifted out of the warm water and plunged into cold, in order to fix the phosphorus, which is then pushed out by means of a wooden or glass rod. The superfluous phosphorus is easily extracted from the vessel in which it was melted, by reason of its conical shape.

Observations.—This apparatus is certainly very effectual for the purpose for which it is intended; nor would it be easy to improve it.

On oxymuriatic Acid. By M. Joseph Mojon.—*Ann. de Chim. No.* 192.

It was frequently observed, in making oxymuriatic acid, that when the acid that was distilled was poured off, and the receiver left for some time exposed to the light, the acid adhering to the sides lost its smell, and acquired that of muriatic ether. In like manner, the oxymuriatic acid itself, being exposed in well-stopped bottles to the sun, ceased to fume, and acquired an ethereal smell; so that the oxygen, instead of being disengaged, entered into new combinations.

Some oxymuriatic acid, which had been exposed to the air for near two years, and had acquired an ethereal smell, was saturated with magnesia, and then distilled. The liquor that came over was rectified by a gentle heat, and yielded a limpid, colourless liquid, of a very strong ethereal smell, and tasting like muriatic ether diluted with water. It did not alter the colour of infusion of mallows, nor did it take fire, being still too much diluted.

It is probable that a portion of ether is formed, at the first distillation of oxymuriatic acid, as was remarked by Giobert, but that the strong smell of the latter hinders that of the former from being perceived.

Observations.—The observations of Giobert were, at the time of their publication, considered as founded on mistake, and the appearances were judged to have arisen from the oil of the fat lute with which the vessels were closed. It is probable that some delusion of the same kind may have taken place in the present instance.

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On the Change of Malt Spirit into Vinegar, and on the Colour of Hemp-seed Oil. By M. RESAL.—*Ann. de Chim. No.* 192.

SOME malt spirit having been mixed with an equal quantity of water, and some beech charcoal added to it, the mixture was forgotten for near a year, when it was found to be changed into a strong vinegar, which retained the bad taste of the beer.

Hemp-seed oil may be distinguished by its appearing of a fine blood-red colour by a direct light, so that when mixed with lint-seed oil it may be easily distinguished as it floats on the surface.

On the Juice of Onions, and on Manna. By Messrs. FOURCROY *and* VAUQUELIN.—*Ann. de Chim. Vol. LXV.*

ONIONS yield a white, viscous, rather opake, strongly scented juice, which turns of a rose-colour by contact of air. This juice is acid, and yields a precipitate by acetate of lead, lime, oxalic acid, nitrate of silver, and potash. When distilled, it yields a watery fluid, with a few drops of oil swimming on it.

The distilled water is slightly acid; it has a strong smell and milky appearance, and yields a pale yellow precipitate by acetate of lead. It contains sulphur dissolved in an essential oil; hence oxymuriatic acid renders it transparent, scentless, and causes it to precipitate nitrate of barytes. When distilled in copper vessels, a pellicle of black iridescent sulphuret of copper is formed in the head of the still.

The portion of onion-juice that remains after this distillation deposited a fawn-coloured matter, from which alcohol separated some oil and sulphur; the remainder of the deposit yielded, on distillation, a black fetid oil, and carbonate of ammonia: hence it was probably a vegeto-animal matter analogous to gluten.

The supernatant liquor was deep brown red, it was sweet, and precipitated acetate of lead yellow; this precipitate melted by the blow-pipe, became black, exhaled the smell of sulphurous acid, and left a globule of phosphate of lead. It dissolved in dilute sulphuric acid, and the solution precipitated by ammonia, or lime-water, let fall phosphate of lime.

Onion-juice being left to ferment, at the temperature of 15 or 20° (59 to 68° Fahr.) did not emit any gas, but it became rose-colour and yellow, depositing a fawn-colour sediment, and was converted into vinegar retaining the smell of onions, so that the essential oil was not altered.

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The sediment that was deposited was very fine, forming a smooth paste, and having a strong smell of onions. It yielded sulphur and an oil by means of alcohol, as oxymuriatic acid added to the alcohol enables it to precipitate nitrate of barytes. The remainder of the sediment has less smell, and exhales the odour of animal substances on hot coals. It does not act as a ferment to a solution of sugar.

The vinegar of onions is yellowish, and has an acid taste, but yet saccharine; it marks 6° hydrom. for acids. When sufficiently concentrated, diverging groups of white needle-like crystals are formed in it. These crystals have a sweet, and yet acid taste; they are mixed with gummy matter and citric acid. Hot alcohol leaves the gum untouched, and deposits on cooling the white crystalline substance in needle-like crystals.

The purified crystals are snow-white, and have a sweet saccharine taste, they are equally soluble in water or alcohol, and burn like sugar. The solution does not ferment with yeast. Nitric acid converts them into oxalic acid, and, if they contain any mucilage, mucous acid. Manna itself, if the mucilage be carefully separated from it, yields only oxalic, and no mucous acid, by the action of nitric acid. It appears, therefore, that these crystals are, in fact, manna.

The raw juice of onions contains only fermentescible sugar, so that the manna found in the fermented juice is a product of the operation. Whence it may be concluded, that a solution of sugar, either when it is too dilute or it contains some peculiar ferment different from yeast, is divided by fermentation into two unequal portions: 1st, vinegar, containing less of the radicals than sugar; 2d, manna, which contains more of the radicals. It is, therefore, not improbable, but that in manna-yielding trees this substance is formed in their saccharine sap, by means of acetous fermentation excited by the gluten of the vegetables; whence new manna is acid, and has the smell of vinegar.

Manna was found to contain not only the crystallizable substance above-mentioned, but also some fermentescible sugar, a small quantity of a nauseous yellow matter, not destructible by fermentation, and which appears to produce the purgative effects of manna, and also a little mucilage.

Juice of melon also contained manna, when fermented, but not before.

244 grammes (7.8 oz. troy) of inspissated juice of onions mixed with 2 litres (quarts) of water, and 30 grammes (0.96 oz.) of dried yeast, fermented at 16 or 20°, emitted carbonic acid gas, and yielded, on distillation, 134 gram. of brandy at 22 deg. hydr. which is equal to 78 gr. (2.34 oz.) of alcohol, at 40°. This indicates, according to Lavoisier, 114 gr. (3.65 oz.) of sugar.

From these experiments, it appears that onions contain, 1. a white, acrid, volatile oil; 2. sulphur combined with the oil; 3.

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a large quantity of uncrystallizable sugar; 4. much mucilage, resembling gum-arabic; 5. a vegeto-animal matter, analogous to gluten; 6. phosphoric acid, and phosphate of lime; 7. a small quantity of citrate of lime, now for the first time found in plants; 8. a pulpy or fibrous substance retaining the vegeto-animal matter.

The emulsion that flows from onions when cut, owes its acridness, and quality of blackening silver, to the union of the oil, sulphur, saccharine substance, and mucilage. Indeed, most poisonous plants are rendered such by the oily and resinous substances they contain; hence, oxymuriatic acid is the surest remedy against their effects.

As to the phosphoric acid, it is probable, that the phosphorus contained in animal manures passes into the plants, where it combines with oxygen, and thus forms this acid.

The calculi composed of earthy phosphates are soluble in onion-juice.

Observations.—The deductions obtained from the analysis of this common root, which are of importance in vegetable chemistry, show the utility of instituting similar researches upon all the bodies which pass through our hands. We recommend to chemists an examination of the thin milky juice of the pepper mushroom, or agaricus piperatus, which, although it has a very acrid taste of pepper, does not in the least affect the polish of steel. This juice dries up in a few days into a pale greenish-yellow substance that retains much of its original acrid taste.

On Almandine, or Greenland Garnet; on the Separation of Iron from Manganese in Analyses; and on Titanium. By M. Gehlen.—*Ann. de Chim. Vol. LXV.*

Grüner and Tromsdorff pretended to have found zircon in almandine, or Greenland garnet; but Klaproth has found only 43 per cent. of silica, 29.5 of oxide of iron, 15.5 of alumine, 8.5 of magnesia, 1.75 of lime, and 0.5 of oxide of manganese, 1.25 being lost.

The succinate of iron not being soluble in water, and that of manganese being very soluble, the different solubility has been used as a means of separating these two metals. Berzelius has since proposed benzoic acid, but Tromsdorff says benzoate of iron is also soluble in water. Dr. John has since employed oxalic acid, and the acidulous oxalate of potash, which Simon says is better than succinic acid, because the oxalate of iron is not so bulky as the succinate.

It has been found by Simon, that titanium is not precipitated from its solutions by tannin, or the substances that contain it.

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On the Use of a Species of Moss for stuffing Mattresses. By M. PARMENTIER.—*Ann. de Chim. Vol. LXV.*

THE moss recommended by M. Parmentier is the hypnum crispum of Linnæus, which is of a good length, and has an aromatic scent that is not lost if it be dried in the shade.

Neither sweat nor urine causes this moss to ferment, as happens to flocks; and if its germination be apprehended, it might be dipped in lime-water previously to its being used, in order to destroy the vital principle.

This moss may also be used for package of tender articles, or for caulking vessels.

On Turquoises, urinary Calculi, artificial Tannin, and a new Re-agent for Acids or Alkalies. By Dr. G. MELANDRI.—*Ann. de Chim. Vol. LXV.*

LAGRANGE announced that the colouring matter of turquoises is phosphate of iron; but it is probable that they are sometimes coloured by oxide of copper, as turquoise is always found in copper-mines. The presence of phosphate of iron may derive its origin from the blood, which is coloured by that salt.

Alemani has found some urinary calculi that contain 51 per cent. of pure magnesia, 20 of silica, 11.84 of phosphate of iron, 4 of carbonate of magnesia, and 3.16 were lost in the process.

The artificial tannin of Hatchett is not an oxide of carbone, as the azote of the nitric acid probably enters into the composition of the tannin, as it does into animal charcoal, and also some hydrogen.

In analyzing atropa belladona, a new vegetable salt, viz. oxalate of magnesia, united with free oxalic acid, was found in its leaves, as also oxalate of lime, muriate of potash, a soft green resin, animal extract, mucus, and oxygenizable extract. If alcohol be poured upon the expressed juice of the ripe fruit of this plant, which is purple, mucus is precipitated, and a tincture is obtained, which, when so diluted with water as to lose its purple colour, becomes green with alkalies and red with acids. After some time the tincture changes to a yellow colour, but without losing its power as a re-agent.

The Supplementary Number (XXII.), *with Title, Contents, and Index, to complete the fifth Volume, will be published on the 1st of March* 1810.

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RETROSPECT

OF

PHILOSOPHICAL, MECHANICAL, CHEMICAL AND AGRICULTURAL DISCOVERIES.

SUPPLEMENT 1809.

CHEMISTRY AND MINERALOGY.

On Phosphorus obtained from Seeds by Distillation; and on the Decomposition of Phosphates by Charcoal. By Mr. THEODORE DE SAUSSURE.—*Ann. de Chim. Vol. LXV.*

FOURCROY and Vauquelin, in their memoir on the soft roe of fishes, contradict Margraff's assertion of his obtaining phosphorus from mustard-seed by distillation. Yet this fact does not depend only on the single testimony of Margraff; he speaks of Albinus and Hoffman, who had also obtained phosphorus from mustard and other seeds, by distillation at a very high temperature, and corroborates their testimony by the facts he had himself observed.

Intending to burn some kidney-beans to ashes in a platina crucible, they were suddenly exposed to a violent heat, the coal and ashes agglutinated together, and a hole was made in the crucible where the charcoal was in contact with it. This effect was not at the time attributed to the real cause.

1039 grammes (about 1lb. av.) of wheat were distilled until the retort had been kept slightly red for three hours: the charcoal, which weighed 250 gram. was then re-distilled in a smaller retort, and in a furnace that produced a more violent heat, so that the luting was entirely melted off the retort. A white smoke smelling of phosphorus filled the receiver, and on opening the cooled vessels a thin layer of phosphorus was found in it. It is probable, that, if this heat had been employed at first, the phosphorus would have escaped with the carburetted hydrogen gas.

Wheat charcoal being treated with water yielded a ley that rendered syrup of violets green, on account of its containing potash.

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Hence, no free phosphoric acid was present, neither could any phosphate of ammonia be contained in the charcoal. It is, however, certain, that the ashes of all seeds hitherto examined contain phosphate of potash, although none was found in this ley, as it was probably retained in the pores of the charcoal.

Chemists have advanced that neither phosphate of potash, or of soda, or of lime without an excess of acid, are decomposed by charcoal. Pure potash was, therefore, neutralized by phosphoric acid, and an excess of alkali was then added; this phosphate being dried and ignited, 30 grammes (about an oz.) of it were mixed with twice its weight of powder of beech [charcoal] which had been previously ignited, and on distillation by a very violent heat, 2 grammes and an half of phosphorus were obtained, besides some that adhered to the neck of the retort, or rendered the liquid product turbid, or was carried off in the phosphuretted hydrogen gas. The phosphate, by calculation, held 4.8 gr. only.

Crystallized phosphate of soda lost 59.06 per cent. by being ignited. 30 grammes of the calcined salt were distilled with beech charcoal, and yielded in like manner 2 and an half gr. of phosphorus, besides the portion that was not collected in a pure form.

Phosphate of lime prepared from the nitrate of that earth, and previously ignited, was distilled with charcoal; but the furnace employed in the preceding operations, was not sufficiently powerful to drive the phosphorus over. A similar mixture of 10 grammes of phosphate of lime, with charcoal, was put into a Hessian crucible with a platina cover, and thus buried in powder of charcoal in another crucible, with an earthen cover. The apparatus was placed for two hours in an excellent wind furnace. The platina cover was melted; the residuum, dissolved in muriatic acid and precipitated first by ammonia, and then by carbonate of ammonia, yielded first 4.52 gr. of undecomposed phosphate, being first dried and ignited, and then 5.22 of carbonate of lime, so that about half the phosphate was decomposed.

6.26 grammes of transparent crystals of native phosphate of lime, heated in the same manner with twice its weight of charcoal, left 4.95 gr. of undecomposed phosphate, and 1.22 gr. of carbonate of lime was obtained.

20 grammes of beech charcoal treated with muriatic acid yield only 0.005 gr. of phosphate of lime, and 0.006 gr. of carbonate of lime.

It is evident, from these experiments, that all animals and vegetables would yield phosphorus, or, at least, phosphuretted hydrogen gas, by a sufficient heat, as they all contain phosphate of lime, and charcoal. And the quantity of this phosphate that may be obtained by incineration will vary according to the heat employed in the operation.

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Observations.—The decomposition of the phosphate of lime by treatment with charcoal, made a great noise among chemists some years ago, in consequence of the phosphorus proceeding from it, combining with iron in the charcoal, or crucible, and the phosphuretted iron having been considered as a metallization of the earths.

In our observations on the original paper of Fourcroy and Vauquelin (see p. 291), we defended the accuracy of Margraff's experiments, and ascribed the failure of the Parisian chemists to the same cause to which it is ascribed by M. Saussure.

On the vegetable Acids that are found saturated by Potash and Lime, in Plants. By Mr. Henry Braconnot.—*Ann. de Chim. Vol. LXV.*

Bergmann and Scheele fixed their attention on those vegetable acids that occur either uncombined, or only saturated in part, but have neglected the other combinations into which those acids enter.

As nitrate of lead decomposes almost all the salts formed by vegetable acids, it was employed to obtain them; and those plants were chosen that contained the greatest quantity of potash, because the precipitates were less coloured, more abundant, and retained less of the mineral acids.

Aconitum lycoctonum in flower pounded with water, yielded an acrid, acidulous juice, which, when boiled and filtered, was brown, passing into red. It reddened tincture of litmus, and yielded a precipitate by nitrate of lead, that was partly soluble in distilled vinegar. The supernatant liquor, which keeps its colour, being evaporated yields nitre; or, if potash be added, deposits a fine orange sediment. The juice is also precipitated by muriate of tin and oxalate of ammonia, but not by nitrate of barytes. A part of the precipitate thrown down by nitrate of silver is soluble in nitric acid, the remainder is muriate of silver. Lime-water throws down coloured flakes; sulphate of iron, or nitrate of copper, a gray precipitate. 1200 grammes of the fresh plant left only 192 of the dried, which being burned, yielded 18.6 of ashes, containing 10.5 of carbonate of potash, mixed with a little muriate, and 8.1 of insoluble substances. The other plants, mentioned in this paper, contain nearly the same proportion of alkali in their ashes. The precipitate obtained by adding nitrate of lead to the juice was diffused in water, sulphuretted hydrogen gas was passed through it, and the liquor was filtered and evaporated to the consistence of a syrup; it yielded a very sour crystalline mass, soluble in water or alkohol, and becoming very white by purification. This acid neither effloresced nor deliquesced in the air; by heat it puffed up, but much less than malic acid does.

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An excess of lime water threw down a precipitate soluble in vinegar, and in an excess of its acid. The acidulous salt yielded, by spontaneous evaporation, a jelly, which, when dried, left a white residuum, that was separated by water into two portions, one in confused crystals, with excess of lime, while the supernatant liquor had an excess of acid. Potash united with the acid of aconitum produced an uncrystallizable deliquescent salt. Ammonia yields an acidulous salt in very short flattened prisms, which throws down a precipitate soluble in acids from nitrate of barytes, but it does not precipitate the nitrates of strontian, or of lime. This acid does not precipitate nitrate of lead, but it throws down white precipitates soluble in distilled vinegar from acetate of lead, or nitrate of quicksilver. Muriate of tin separates a colouring matter from this acid, which has some resemblance to the malic, and probably saturates the potash which is yielded abundantly by aconitum.

The clarified juice of delphinium elatum (Siberian bee larkspur) reddens tincture of litmus, and precipitates oxalate of ammonia, lime-water, and nitrate of lead. The last precipitate, washed and treated with dilute sulphuric acid, yielded by evaporation a granulated, crystalline acid mixed with extract. This acid does not alter the nitrates of lime, barytes, strontian, lead, or silver. It throws down from acetate of lead, a precipitate soluble in vinegar, and from muriate of tin a coloured precipitate. It forms with lime a salt which is soluble when it has an excess of acid, and insoluble when the base is in excess.

Ranunculus aconitifolius (mountain ranunculus), thalictrum flavum (great meadow rue,) clematis recta and viticella (upright and purple virgin's bower), probably contain the same acid.

The clarified juice of salvia sclarea (clary) is rather deep, and reddens tincture of litmus. Nitrate of barytes precipitates it, and the precipitate is partly soluble in nitric acid. Potash changes it to yellow, oxalate of ammonia and muriate of tin precipitate it. Nitrate of lead throws down a white precipitate, soluble in distilled vinegar. This precipitate, being decomposed by dilute sulphuric acid and the liquor evaporated, formed a crust containing fine needle-like crystals, of a vinous and weak acid taste. It requires 80 parts of water at 40° (104° Fahr.) to dissolve one part of this acid, which is very soluble in alkohol. It is decomposed by heat, and one part is sublimed. It does not precipitate lime-water, and forms a soluble salt with carbonate of lime. With potash it forms divergent prisms, slightly soluble in water: nitric acid added to this neutral solution produces a fine white cloud in it. This acid decomposes nitrate of silver, and precipitates muriate of tin, but not nitrate of lead. So that it strongly resembles benzoic acid, although it has not the balsamic smell. Melissa (balm), and several other labiated plants, which abound in potash and hydrogen, probably contain this acid; and this dis-

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covery serves to explain the existence of benzoates in the urine of cattle.

The clarified juice of ruta graveolens (rue) is a deepish yellow, and reddens tincture of litmus. Nitrate of lead throws down a white precipitate, and potash separates from the supernatant liquor a beautiful deep yellow precipitate. Muriate of tin precipitates it sulphur-yellow. Sulphate of alumine has no effect till potash is added, and then a citrine-yellow precipitate falls, and the liquor becomes clear. Lime-water precipitates it of a fine yellow. Oxalate of ammonia and nitrate of barytes separate a quantity of lime from it. The precipitate from nitrate of lead decomposed by dilute sulphuric acid yielded an uncrystallizable and deliquescent acid, which left much charcoal on being dried, and burned. It was not soluble in alkohol. Lime-water added to excess yields a flaky precipitate, soluble in an excess of acid. With carbonate of lime it forms an acidulous uncrystallizable salt; with ammonia it forms confused crystals, decomposed by nitrate of barytes, but not by the nitrates of lime or strontia. It does not alter nitrate of lead, or muriate of tin, but it precipitates nitrate of quicksilver ad minimum, and acetate of lead; the latter precipitate is soluble in distilled vinegar. The precipitates produced by nitrate of silver in combinations of this acid, become red instantly on exposure to the light, then black, and afterwards of a metallic brilliancy. So that rue contains malic acid, potash, lime, and a yellow colouring matter. Portulaca (purslane) contains much potash neutralized by malic acid.

The juice of eupatorium cannabinum (hemp agrimony) reddens tincture of litmus; oxalate of ammonia, and nitrate of barytes, precipitate it; part of the latter precipitate is soluble in nitric acid, the undissolved residuum is sulphate of barytes. Lime-water separates coloured flakes. The coloured precipitate produced by nitrate of lead being decomposed by sulphuric acid yielded a brown uncrystallizable acid, which precipitated acetate and nitrate of lead, but not nitrate of lime. Lime-water throws down flakes containing all the colouring matter. This acid is probably malic acid, containing some phosphoric, and much extractive matter.

Tanacetum vulgare (tansey), crysanthemum segetum (corn marygold), spilanthus oleracea, which contains a deal of nitre, xymenesia encloides, and milleria contrahierba, yield ashes, that are very fusible and caustic.

The clarified juice of nicotiana rustica (English tobacco) reddens tincture of litmus, and is precipitated by oxalate of ammonia, nitrate of barytes (the latter precipitate is partly soluble in nitric acid), lime-water, muriate of tin, and the nitrates of quicksilver, silver, and lead. On decomposing the precipitate from the last nitrate, by sulphuric acid, a red, sharp-tasted, but agreeable, uncrystallizable acid was obtained, which puffed up, and was de-

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composed by heat. It drew out in threads between the fingers, and formed a varnish that attracted moisture. It did not precipitate nitrate of lead, but threw down from acetate of lead a precipitate soluble in vinegar; and was indeed nitric acid.

Nicotiana tabacum (Virginia tobacco), paniculata (Brasilian tobacco), and undulata, as also solanum tuberosum (potatoe, in the stalks of which nitre is found, while malic acid has been found in the fruit of many solanums), and some capsicums, furnish ashes stronger than some common kinds of potash, and probably contain much malic acid combined with the alkali.

The clarified juice of mirabilis jalapa (marvel of Peru) is deep red, and reddens litmus-paper. It was rendered turbid by oxalate of ammonia and lime-water. The precipitates by muriate of tin, and the nitrates of lead and silver, are small in quantity, and draw down all the colouring matter; that by nitrate of quicksilver is more in quantity and less coloured. When this precipitate was analyzed by sulphuretted hydrogen, it was found to contain much muriatic acid, malic acid, sulphuric acid, and extractive matter. On evaporating the juice of this plant it yields plenty of nitrate of potash.

The water in which spinacia oleracea (spinach) has been boiled, reddens tincture of litmus. It is not rendered turbid by oxalate of ammonia, but lime-water throws down a yellow precipitate, and the liquor is left colourless. Muriate of lime precipitates it abundantly; but muriate of tin, and the nitrates of barytes and strontia, slightly. Nitrate of silver yields a precipitate partly soluble in nitric acid; the residuum is muriate of silver. Nitrate of lead throws down a white precipitate; and potash added to the supernatant liquor separates a yellow precipitate, which leaves the liquor colourless. The white precipitate from nitrate of lead became yellow when washed, and being decomposed by sulphuric acid, it yielded crystals of oxalic acid: the supernatant liquor evaporated to dryness, and treated with alkohol, yielded a viscous acid, not crystallizable, but forming an acidulous salt with lime, that contained the malic and phosphoric acids. Spinach was exhausted by long boiling, treated with muriatic acid, and then potash was added; a white precipitate fell down, which, on being decomposed by carbonate of potash, yielded oxalate of potash. The laxative virtue of spinach, therefore, arises from the oxalates of lime and of potash, together with malate and phosphate of potash. It is probable that the oxalate of potash is in great quantity in the stems of rheum compactum (plain-leaved rhubarb) and atriplex hortensis (orach), as they yield a very large proportion of alkali on incineration.

The clarified juice of tropæolum majus (nasturtium) is pale amber colour, and reddens tincture of litmus. Oxalate of ammonia separates lime from it. Lime-water throws down yellow flakes. Muriate of lime causes a slight precipitate. The nitrates of quicksilver, silver, and barytes, and muriate of tin, throw

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down white precipitates mostly soluble in nitric acid. Red sulphate of iron renders the juice green; and yet if a prussiate be added, a whitish precipitate is obtained. Nitrate of lead throws down a white precipitate, which becomes brimstone-yellow, by washing, and prase-green, when dried. This precipitate, decomposed by sulphuric acid, yielded a red, uncrystallizable, viscid acid, attracting moisture, which appeared to be phosphoric acid mixed with malic; but the plant also contains the nitric, for when the juice was evaporated, a gelatinous mass was obtained, which contained numerous crystals of nitre.

Dry tropæolum was burnt to charcoal, washed, reduced to ashes, and elixiviated. The ley, on being evaporated, exhaled a penetrating odour of ammonia, and left 94.63 per mille of a mixture of carbonate of potash, the phosphates of potash and lime, and the sulphate and muriate of potash. The insoluble residuum weighed 43.37, and was for the most part soluble in nitric acid: nitrate of lead threw down from this solution, phosphate of lead nearly equal in weight to the matter originally taken up. It is, therefore, probable that the flashes of light observed by Linnæus's daughter in the sexual parts of this plant arise from phosphorus, which burns as soon as it is produced.

Haggren has observed these luminous appearances in several plants: as, 1. calendula officinalis (marygold); 2. tropæolum majus; 3. lilium bulbiferum; 4. tagetes patula, and erecta (French and African marygold); 5. and sometimes in helianthus annuus, (dwarf sunflower). These phenomena, and the light that some insects emit from the last segment of their abdomen, arise doubtless from the same cause; and it evidently appears, that phosphorus has important effects in the generation of some beings; especially as it has been found pure in the soft roe of fish.

Juice of ricinus communis (palma christi), on adding nitrate of lead, yields a white precipitate, which, when decomposed by sulphuric acid, affords malic acid. This plant, like all those previously mentioned, yields a very large quantity of potash.

Much pains have been taken in finding the best mode of making potash, but none in choosing the plants which contain the largest proportion of it.

The malic acid appears to be the most common of native vegetable acids; saturating the potash found in plants, and forming with lime an acidulous salt, to which the acid taste frequently observed in their juices is owing. The next common acid in vegetables is the oxalic; the nitric and phosphoric acids are also more common than they were thought to be.

The editors of the Annales de Chimie observe that Vauquelin has obtained crystallized citric acid from aconitum; but it is probable that it also contains a small quantity of malic acid, and perhaps of the acetous.

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The delphinium probably contains also citric acid.

The acid of rue seems to be malic acid: this plant contains also a fine greenish-yellow colouring matter, which may be applied to wool, silk, or cotton, by means of alum; but Vauquelin is apprehensive, from his trials, that the colour is not solid.

Paumier and Cabale, pupils of Vauquelin, made, some years ago, an analysis of tobacco, and obtained vegeto-animal matter, malic acid, nitrate of potash, lime, potash, &c.; but this analysis has not been published.

Fourcroy and Vauquelin have found a considerable quantity of oxalic acid united with lime and potash, a very large proportion of nitrate of potash, and very little colouring matter, in the banana.

The odour of ammonia, which Braconnot observed in the evaporation of the ley from the ashes of tropæolum, appears to arise from prussiate of potash formed during the incineration.

Fourcroy and Vauquelin have found phosphoric acid, and phosphate of lime, in onions; and the latter has discovered that acid in almost all the kinds of corn.

Erigeron Canadensis has been found by Lagrange to yield more potash than any other plant.

Account of some galvanic Experiments. By Mr. Gehlen.—*Ann. de Chim. Vol. LXV.*

The best method of obtaining the metal of potash, is to put a globule of quicksilver upon potash, and insert the negative wire in it. By this means an amalgam is obtained, which grows solid on cooling, and, on being put into water, emits gas copiously without taking fire.

Bucholz has continued his experiments on galvanic circles formed of a metal, the solution of that metal, and pure or acidulated water.

Green muriate of copper yielded a precipitate of white muriate of copper.

Nitrate of copper deposited a thin crust of copper upon the metal, and also globules, which appeared to have one or two openings in their sides when viewed with a lens.

Nitrate of silver deposited the metal in lines of brilliant grains, which, by a high magnifying power, appeared to be cubes truncated at the corners.

Nitrate of lead deposited a group of small leaves, with many small insulated crystals interposed between them.

In all these experiments there is a spot, which is sometimes 3-4ths of an inch diameter, on the metal plate, on which neither precipitation nor oxidizement takes place. The oxidizement is stronger where the liquid and metal come in contact with the air. The action of the circle is augmented when the water is acidu-

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lated; indeed, that formed with acetate of lead does not act when pure water is used.

It is necessary also, that the solution should not be altered by the metal immerged in it, and that the two liquids should differ considerably in specific gravity, that they may not mix too soon; and lastly, the metal must be oxidizable by the second liquor, but this oxidizement ought not to be too rapid, lest it should mix the two liquids.

Pfaff has made many galvanic experiments with great care, employing a condenser, both whose plates were covered with amber varnish. This condenser was so sensible, that the action of a single pair of zinc and copper plates caused the gold leaves of an electrometer to diverge an inch; the leaves being 2 in. long, and 3-4ths in. broad.

He has observed, that sulphuret of potash acts as a metal, and therefore piles may be composed of three unmetallic liquids.

On Glauberite, a new fossil Salt. By Mr. Alexander Brongniart. *—Journ. des Mines, No.* 133.

This mineral is the only one hitherto known, in which two complete salts appeared to be so strongly combined together as to form a well-characterized mineral species. It was found among the minerals brought from Spain by Mr. Dumeril, where it was taken for gypsum, although its form is very different.

The crystals are very low oblique prisms, with rhomboidal bases, of 70 d. and 104 d. The incidence of the base upon the adjacent sides is 142 d. and 38 d. The inclination of the edge, formed by the two planes of the obtuse angles upon the base, is 154 d. all from actual measurement. The bases are neat and brilliant, the lateral faces streaked parallel to the bases. They have very neat joints parallel to the bases, and other joints, not so neat, parallel to the edges of the bases, and inclined upon the former about 104 d. In other directions the fracture is glassy.

The crystals are neat, transparent, homogeneous, generally of a topaz-yellow, but sometimes limpid; they remain solid and transparent, unless they are moistened. They are not marked by the nail, but they do not scratch calcareous spar.

This mineral, exposed to a sudden heat, splits, decrepitates, and forms a white enamel. It becomes milk-white and opake when put in water; on being taken out and dried, the white crust falls to powder, and leaves an unaltered nucleus. The specific gravity is 2.73.

Hauy observed, that it refracted a single image through one of its bases, and an artificial face inclined to that base; that its powder did not render syrup of violets green; and that it ac-

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quired such a very feeble resinous electricity by friction, that it could not be observed unless insulated.

Glauberite, exposed to the heat required for its fusion, lost only 0.003 of its weight; and it did not yield any perceptible quantity of water when distilled with iron filings, as is the case when melted barytes is distilled with that metal.

On a careful analysis it was found to contain 51 per cent. of anhydrous sulphate of soda, and 49 of anhydrous sulphate of lime, independent of 0.003 of water, and some other accessory substances, which are interposed between the molecules, as clay, muriate of soda, and a small quantity of iron.

Glauberite has only been found at Villarubia, near Ocana, in New Castille, disseminated in rock-salt, to which it is prior in formation, as its crystals penetrate those of the salt. Glauberite effloresces by moisture, because the water dissolving the sulphate of soda, changes that of lime into a spongy substance.

The anhydrous sulphate of lime at Pesey contains 40 per cent. of lime, and calcined sulphate of the neighbourhood of Paris contains 42. Several varieties of the latter were found to contain 21 per cent. of water.

Mr. Berthier has observed, that a fifth part of the scales that are deposited in the evaporating pans of salt-houses are composed of anhydrous sulphate of soda; and as these scales contain scarcely 7 per cent. of water, their formation in a saline solution is a singular fact.

On uniting Pycnite and Topaz, as being the same mineral Species.—*By Mr.* Rene' Just Hauy.

The analyses of these two stones agree in some respects, as appears by the following table:

Topaz,	Saxon.	Klaproth.	Sil. 35,	Al. 59,	Ac. fluor. 5,	Loss, 1.
——	Brasil.	——	44·5,	47·5,	7,	0.5.
——	——	Vauquelin.	30,	50,	18 to 20.	
Pycnite,	——	Bucholz.	34,	48,	17, Ir. and mang. 1.	
——	——	Vauquelin.	30,	60,	6, Lime, 2; Water, &c. 1.	

The only cleavage hitherto perceived in topaz has been one perpendicular to the axis, so that the primitive form was thought to be a rhomboidal prism; but in this instance a secondary form had been taken for the primitive.

Oblique joints have lately been discovered, by means of a strong light, in topaz, so as to show that the true primitive form is a rectangular octaedron, divisible parallel to the common base of the two pyramids of which the octaedron is composed. The incidence of the opposite pairs of planes upon the base is 61 d. 21 s. and 44 d. 1 s. and, of course, upon each other the double of these angles.

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A large crystal of pycnite (schorliform beryl of Jameson) has been discovered, in which joints were observed which pointed out a rectangular octaedron similar to that of the topaz: and the hexaedral prisms of pycnite are not regular, as has been supposed, as two of the angles are of about 124d. and the other four of 118d. The crystals did not show any joints parallel to the axis; those perceived in the common crystals are a deception, as they arise only from the crystalline groups being divisible almost indefinitely into fine needles.

Crystals of pycnite are usually very easily frangible in a transverse direction, in which they differ from topaz; but some whitish emeralds differ in the same manner from Peruvian and Siberian emerald. The specific gravity is the same. The more glassy the pycnite is, the less brittle, and its fragments scratch quartz almost as easily as those of topaz do. Common pycnite is not electric by heat, but from a specimen of a more glassy texture than usual, electric effects were obtained; so that there can be no doubt but that pycnite and topaz ought to be united into one single species.

Observations.—We perfectly agree with this experienced mineralist in thus uniting pycnite with topaz.

On volcanic Products. By Mr. Louis Cordier.—*Journ. des Mines, No.* 132.

In the former paper was related the discovery of a new species of sand, which may be called octaedral titanized iron; the next step is to examine the minerals which furnish this sand.

That class of volcanic rocks, whose origin is still contested by the German school, will be examined apart. These rocks differ only from the others in their insulated position on hills, where there is no appearance of any crater. The followers of Werner appear to be far behindhand in regard to their knowledge of these substances, as they enumerate klingstein, basalt, and lava, as separate species. But even the newest currents of lava exhibit in their internal part either basalt, or klingstein; and the species lava appears to be only the porous crust that is formed by the coagulation of the incandescent matter. The coincidence of these species is ascertained by their contexture, and by the agreement of their constituent parts.

Titanized iron may be discovered by a lens, or the magnetic bars: 1. In the cinders and gravel of volcanoes; in amorphous grains, which might be confounded with black, opake grains of pyroxene: of these the ashes of Vesuvius contain 2 or 3 per cent. 2. Porphyritic lithoid lavas contain this substance sometimes in large grains or crystals, which have been mistaken for specular iron ore, or hæmatites; but the first is very rare, and the latter

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never found in volcanic products: in general it requires a lens, although there are few lavas of this kind, in which this mineral cannot be distinguished by its metallic splendour, or, if devoid of that, by the use of the magnet. 3. The magnet must also frequently be used to discover the nature of those large grains of titanized iron, which are so remarkable in granitoid lavas.

The presence of titanized iron, in such small particles as to be cognizable only by the magnetism of the mass, may be sometimes rendered manifest by pulverizing the lava, and applying a magnet to the powder.

The application of muriatic acid to some of those varieties of volcanic products from which titanized iron could not be separated by mechanical means, so as to deprive them of their magnetic properties, abstracted from them the elements of titanized iron, and very nearly in the same proportion, only some part of the earthy substances was acted upon by the solvent. After this separation of the elements of titanized iron from volcanic products, so as to deprive them of their magnetic properties, if the residuum was examined, by means of caustic potash, it yielded iron combined with a small portion of manganese.

Titanized iron contains 80 per cent. of oxide of iron, 15 of oxide of titanium, and a little manganese and alumine. It is the cause of the magnetic properties of volcanic substances, except when they contain sublimed specular iron ore, which is very seldom the case. The proportion varies from 2 to 18 per cent.; the basaltic or granitic lithoid lavas containing the most. The only varieties of lava in which it has not been found are those varieties of vitreous lava, pumice, and volcanic tophus, which are not magnetic, and melt into a white glass.

The separation of the oxide of titan is very difficult; but if it be only required to ascertain its presence, the muriatic solution may be evaporated to dryness, at the risk of precipitating some of the iron and alumine. The residuum treated with the blow-pipe produces the same effects as if it was pure oxide; but a small quantity of iron alters the colours of the precipitates by means of reagents. To get rid of the iron, the residuum must be calcined, and digested speedily in muriatic acid; the undissolved part collected upon a filtre, and melted with alkali, in order to obtain a solution.

Lavas do not contain nickel, nor iron in a metallic state; as to chrome, it was not looked for.

Observations.—An abstract of Mr. Cordier's first paper may be found at p. 244.

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On the Analogy between Diopside and Pyroxene. By Mr. HAUY. *—Journ. des Mines, No.* 134.

THE minerals, to which their discoverer, Mr. Bonvoisin, had given the names of mussite and alalite, appeared, upon examination, to be only a single species, to which the name of diopside was applied.

The primitive form assigned to this species was very nearly the same with that of pyroxene (augite); the only difference being, that the planes of the prism of pyroxene were inclined to each other at 92 d. on one side, and 88 d. on the other, whereas in diopside the prism was rectangular. The crystals of diopside have generally eight sides, so that it is more difficult to measure the inclination of the primitive faces, than when these faces are contiguous; and it must be owned, that the appearance of the two minerals is so very different, that a kind of prejudice was excited in favour of the apparent difference in their primitive forms.

Since that time Mr. Jurine has brought to Paris some large insulated crystals of diopside, which, on measurement and subsequent calculation, showed that their primitive form was entirely similar to that of pyroxene.

The diopside was also distinguished by admitting two subdivisions in the direction of the two diagonals of the base, while pyroxene was thought to admit only that in the direction of the longer diagonal, but the second has since been found among the crystals of pyroxene from Vesuvius and Arendahl.

The hardness of diopside and pyroxene is nearly the same, as is also the specific gravity: the principal remaining differences are their geological situations and appearance, which latter is very striking. Pyroxene has lately been found, imbedded in felspar, at Arendahl in Norway, in a formation which seems primitive, and by no means volcanic. Small transparent crystals of pyroxene have also been found at Vesuvius of a green colour. Many crystals of pyroxene are very glassy and brilliant, others are only lamellar, and some from Arendahl appear like flakes of brown mica.

To this may be added, that the coccolite of the Danish mineralists has been found to be granular pyroxene; and it is probable that malacolite or sahlite ought also to be considered as a variety of pyroxene. Now coccolite apparently passes on one side into pyroxene, and on the other into sahlite; and some specimens of sahlite have nearly the appearance of that variety of diopside called mussite by Bonvoisin: so that, although the extremes are very different, they are connected by intermediate steps.

It is well known, that many very different substances have the same integrant molecules, but then it is usually one of those figures which may be considered as limited, such as the cube or

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regular octaedron: and if it should not be limited, the other physical characters will suffice to determine the species without any equivocation. But the properties of diopside and pyroxene tend to confirm their union in a single species. If chemistry should hereafter show any essential difference between these two substances, it will be a very remarkable exception to the crystallographic method of classification. It may, however, be considered as proved, that the same substance never has more than one form of its integrant molecules. Borax is an example of the other case, as its crystallization is analogous to that of pyroxene, but its solubility and taste are sufficient to distinguish it.

Bournon has given the inclinations of the planes of sahlite rather different from those of pyroxene; and it is in consequence of his determinations, and also because the crystals of sahlite at Paris are too imperfect to obtain accurate measures, that the union of sahlite with pyroxene is still considered as doubtful.

Observations.—An abstract of Bonvoisin's description of mussite and alalite may be found in our second volume, p. 353.

Analysis of several ancient Bronze Instruments, &c. By Mr. KLAPROTH.—*Journ. des Mines, No.* 135. *From Gehlen's Journ. No.* 15.

THE bronze was digested in nitric acid, of the specific gravity of 1.220; and the oxide of tin being collected, it was estimated to contain 80 per cent. of the metal.

An ancient sword, found in Brandenburg, was composed of 89 per cent. of copper, and 11 of tin. The brilliant polish it took showed the hardness and tenacity of the metal. These swords, and other cutting tools, were not forged, but cast, and an edge given them by grinding.

Two instruments, in the shape of sickles, but whose use is disputed, contained, the one 15 per cent. of tin, and the other 13.

Some fragments of an elastic ring contained only 9 per cent. of tin. This ring appeared to be similar to another, which did not form a perfect circle, but of which the two ends came together with so much elasticity that they were difficult to separate. This ring took a most superb polish: its use is unknown, but the elasticity of the bronze merits attention.

A Greek broche contained 11 per cent. of tin.

Some small nails were formed of 2.25 of tin, and 97.75 of copper.

An ancient cup, found in a Greek sepulchre at Naples, and whose interior surface had lost very little of its polish, contained 14 per cent. of tin, and 86 of copper. It was very thin.

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A great number of other specimens yielded from 10 to 13 per cent. of tin.

The four horses at Venice were supposed to be formed of copper, on account of their being gilt, as copper is much easier gilded than bronze. In reality the metal contains only 0.35 per cent. of tin, or 7 lb. in 1000: so that it is probably an accidental admixture.

Bronze is at present used only for cannons, bells, and statues; but it ought to be examined whether bronze is not far preferable to brass for kitchen furniture, as it is much less liable to become oxidized. The ancients used it for these purposes, although they were well acquainted with the danger attending the internal use of oxide of copper, as well as its use in curing wounds.

An ancient Chinese coin consisted of 47.75 per cent. of copper, 15.25 of lead, and 8 of tin. A modern one consisted of 56,5 per cent. of copper, 4 of lead, and 1.5 of tin.

Cinnamon-stone, treated on charcoal by the blowpipe, melts gradually and slowly into a shining glassy bead, which is externally of a deep green gray: heated in a platina crucible, it only became paler than before. As it nearly agrees in its constituent parts and outward appearance with vesuvian (or idocrase), it may be arranged with it.

Some zircons, from the northern circars of Bengal, were found to contain 64.5 per cent. of zircon, 32.5 of silica, and 1.5 of oxide of iron. It appeared also, that when zircon is precipitated by the alkaline carbonates, it absorbs a small quantity of carbonic acid, and that carbonate of zircon contains 51.5 per cent. of zircon, 7 of acid, and 41.5 of water.

Observations.—We cannot but consider the use of bronze vessels in the kitchen as far superior either to brass, tinned iron, or tinned copper, both in respect to the diminution of danger, and the superior strength; and the introduction of them into use would be an act of patriotism, as they would consume a great quantity of tin.

We have omitted the analysis of cinnamon-stone, as having been already mentioned in our last number, p. 476.

Analyses of some Products of Iron Furnaces and Forges. By Mr. Berthier.—*Journ. des Mines, No.* 135.

It will be important to compare these analyses with those given by Mr. Guenyveau, p. 481. The substances analyzed were procured from the smelting furnaces of Mont Blanc and Allevard, and from the steel manufactory of Rivieres. Mr. Descostils has examined the ores made use of in them. See Retrospect, vol. ii. page 334.

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The scoriæ from the high furnaces of St. Helena vary in their appearance, so that the smelters can only form a judgment of them by considering the quantity of small iron shot that they contain. In general, the smeltings at Mont Blanc go on well, and the scoriæ are rarely so thick as to incumber the basin: no flux is added.

Black porous and light scoriæ, formed when the smelting goes on well, contained 53 per cent. of silica, 15 of lime, 1 of alumine, 8 of magnesia, 11.5 of oxide ad minimum of manganese, and 9.3 of oxide ad minimum of iron.

Green compact scoriæ, formed when the smelting did not go on so well, contained 60 per cent. of silica, 10 of lime, 6 of alumine and magnesia, 19.8 of oxide ad min. of manganese, and 4 of oxide ad min. of iron.

Yellowish-gray compact scoriæ, formed when the smelting did not go on well, contains 71 per cent. of silica, 7.2 of lime, 2.5 of alumine, 3.2 of magnesia, 8 of oxide ad min. of manganese, and 4.7 of oxide ad min. of iron.

The scoriæ from the furnaces at Allevard form irregular masses, which contain imbedded charcoal, grains of pig-iron, and large pieces of the white quartz, which is the vein-stone of the ore, and which is not separated with sufficient care, whilst the scoriæ being already saturated with silica, do not dissolve the whole. If the quartz was reduced into smaller pieces, it would be dissolved, and thicken the scoriæ, so that more flux must be added. The flux is a calcareous tophus, which contains at most 0.04 or 0.05 of clay and vegetable matter. The operation of smelting is very complicated, as the variations in the quality of the ore require continual alteration in the additions.

Homogeneous green porous scoriæ were found to contain 46 per cent. of silica, 10.6 of lime, 6.6 of alumine, 6.6 of magnesia, 10 of oxide ad min. of manganese, and 18.5 of oxide ad min. of iron.

Another specimen contained 54 per cent. of silica, 8 of lime, 4.5 of alumine, 6.5 of magnesia, 13.5 of oxide ad min. of manganese, and 9.3 of oxide ad min. of iron.

Well-melted glassy scoriæ, clouded gray and blue, from whence the quartz had been separated, contained 56 per cent. of silica, 13 of lime, 6.5 of alumine, 9.3 of magnesia, 9 of oxide ad min. of manganese, and 4.7 of oxide ad min. of iron.

No potash could be found in any of these scoriæ.

The furnaces at Mont Blanc and of the Isere are only from 6 to 7 metres (yards) high. Their temperature is much higher than at Creusot; so that the scoriæ retain scarcely any oxide of iron. It is probable that the heat is less at St. Helena, and still less at Allevard, as they retain so much of the oxide.

In the furnaces of Mont Blanc they usually make white, fibrous, hard iron; but at Allevard they prefer very gray, granular iron, soft and easy to be filed. The gray iron is used at Rives to make

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steel, for which purpose it is preferred. The white yields good tough iron, but the steel made from it is reduced into iron in the mere process of tempering it, so that it probably contains the smallest possible quantity of carbone.

White cast iron of St. Helena was found to contain 1.7 per cent. of insoluble residuum, arising from the scoriæ mixed with it, no sulphur or phosphorus, 1 of charcoal, 1.5 of metallic manganese, and 0.1 of copper; in all 4.3.

Gray cast iron of Allevard contained 2.2 per cent. of insoluble residuum, some traces of sulphur, 3 of charcoal, 1.8 of metallic manganese, and 0.2 of copper, but no phosphorus; in all 7.2.

A mixed smelting, of about one third of the first and two of the second, contained no insoluble residuum, sulphur, or phosphorus, 0.5 per cent. of charcoal, 0.4 of metallic manganese, and 0.3 of copper; in all 1.2.

In the departments of Mont Blanc and of the Isere a peculiar process is used for refining pig-iron. It is remelted, rich scoriæ are stirred into it, the mixture is then agglutinated, and formed into a lump ready for drawing out. A large quantity of fuel is used. The basin of the furnace is made anew, and without any scoriæ, which are added during the fusion. Other scoriæ are formed during the operation, which are very fluid, and let out at the eye of the furnace. These scoriæ are black, metalloid, attractible, either porous or compact, granular or lamellar. They contained 23 per cent. of silica, 2 of lime, 1 of alumine, 1 of magnesia, 29 of oxide ad min. of manganese, and 45 of oxide ad min. of iron.

Other scoriæ of the same appearance are formed during the subsequent operations: these contained 19 per cent. of silica, 1 of lime, 1 of alumine, 1 of magnesia, 10.5 of oxide ad min. of manganese, and 51.5 of oxide ad min. of iron.

A third kind of scoriæ were detached from the bottom of the basin after the lump was taken away, and contained 18 per cent. of silica, 14.5 of lime, 1 of alumine, 1 of magnesia, 9.5 of oxide ad min. of manganese, and 61 of oxide ad min. of iron.

The scoriæ of another forge contained 9.5 per cent. of silica, 10.5 of lime, 1 of alumine, 2 of magnesia, 3 of oxide ad min. of manganese, and yielded 56 of iron when assayed.

Another specimen from the same forge contained 8 per cent. of silica, 7 of lime, 0.5 of alumine, 0.5 of magnesia, 3.5 of oxide ad min. of manganese, and yielded 61.5 of iron when assayed.

The scoriæ are mixed with pieces of iron, which were previously separated as much as possible, but some remained in spite of every care.

It is very observable, that the greater part of the manganese existing in the pig-iron is scorified during the refining; it is contained in the greatest proportion in the first scoriæ, and the latter scoriæ are less charged with it, but they contain a considerable

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quantity of oxide ad minimum of iron. The two metals supply one another's places in procuring the fusion. It is probable, the addition of oxide of manganese would cause some of the iron to be reduced.

The presence of manganese is therefore not prejudicial, as it is got rid of during the refining of the iron; and even in the better kinds of cast work, which are annealed in the reverberatory furnace, the manganese is oxidized, and melts along with the scoriæ.

The iron refined in Mont Blanc and l'Isere loses from 20 to 25 per cent.; so that about 12 or 15 per cent. of metallic iron is oxidized and vitrifies with the earths. It does not appear that this loss can be avoided, for it is difficult to form a mixture of earthy substances which is easily fusible at the temperature of the forges. Oxide of manganese might perhaps be used as a flux.

The heaviest scoriæ flung out of the refining-houses are as rich as the most valuable ores, and are said to yield iron of the best quality, so that they might be smelted with ease; but then it would be necessary to alter some customs, and that is always a very difficult task.

The more powerful the furnace, the greater is the proportion of pig-iron obtained in the smelting; but then more fuel must be employed. Every kind of ore requires a particular treatment in this respect. In general a poor ore ought to be smelted at a high temperature, in order not to lose any of the metal it contains. It is therefore absurd to smelt marsh iron ores in low furnaces, or in forges, and to leave in the scoriæ one half more iron than is obtained; and yet this is sometimes done.

The iron of Mont Blanc is soft and tough, but sometimes difficult to forge at a red heat, which may arise from the copper it contains, but it is more probable that it proceeds from the iron not being homogeneous, as the copper is visibly mixed with it. It is said that good iron cannot be made from the pig-iron of Allevard by itself, as it would be hard and red-short; it is, therefore, necessary to add that of Mont Blanc to soften it, and render it tractable. Some iron-masters are attentive to these circumstances, others are perfectly indifferent in their choice, and add unknown foreign iron, or any old iron they can purchase cheap, so that the iron is frequently useless. A specimen of this kind could neither be forged nor welded: it was found to contain 0.3 per cent. of copper, and at least 0.5 of phosphorus, although there was no trace of this last in any of the substances smelted in the high furnaces. It is proper that the old iron should be made serviceable again, but it should not be mixed with other kinds of known reputation.

The steel manufactories of Rives use up almost all the pig-iron of Allevard, as it is principally used to form steel; the remainder of the iron generally comes from Mont Blanc and Franche

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Comté. The careless mixture of any kind of iron produces the inconveniences just mentioned in respect to iron.

The method of making steel at Rives is very simple: the pig-iron is melted through charcoal, the scoriæ are stirred in, and the iron is kept in tranquil fusion at the bottom of the furnace. The scoriæ are run out from time to time, and also the congulated masses on the side.

Some of the scoriæ are porous and light, and contain 29.3 per cent. of silica, 14.3 of lime, 2 of magnesia and alumine, and 10 of oxide ad min. of manganese; they yield 31.4 of iron on assaying them.

Some are very compact, with a brilliant lamellar fracture, and contain 29 per cent. of silica, 8 of lime, 1.5 of magnesia and alumine, and 4 of oxide ad min. of manganese; they yield 44.5 of iron.

The heaviest scoriæ are those which remained the longest in the furnace; they are covered with confused groups of octaedral crystals which are sometimes several millimetres (0.039 in. each) in length. They contain 21 per cent. of silica, 4.3 of lime, 1 of magnesia and alumine, and 3.5 of oxide ad min. of manganese; and they yield 56.2 of iron.

Pig-iron, therefore, parts with the manganese on being converted into steel. Besides the charcoal nothing is found in it, but the copper. Rives steel is of a good quality: many specimens which were in small bars and tempered were excellent; its principal defect arises from the admixture of veins of iron. Great caution is always necessary to prevent this in steel made immediately from pig-iron, and none is taken at Rives; the heating of the pigs in the fire-place of the refinery contributes to increase this defect, and therefore Mr. Baillet has proposed another method (Journ. des Mines, vol. i. p. 21), which it is to be wished were adopted.

In the above analyses, the iron and manganese were separated by means of saturated carbonate of potash; but as this required great care, when peculiar exactness was required, acetous acid was boiled upon the oxide of iron which is flung down, to dissolve the carbonate of manganese which it retains. The solution being gently evaporated, then diluted with water and boiled, the oxide of iron, which was taken up by the acetous acid, is deposited, and only the manganese remains dissolved.

It appears certain, that the oxides of iron and manganese are ad minimum in the scoriæ. It is easy to calculate the quantity of iron from the red oxide; but the oxide of manganese can only be computed from the quantity of the carbonate. The brown oxide obtained from the carbonate by calcination varies according to circumstances.

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The solution of manganese generally contains iron, cobalt, and copper, all of which are precipitated by an alkaline hydrosulphuret before the manganese, which at length falls down of an Isabella yellow. The solution is then precipitated white by the prussiates. Copper, if present, renders this precipitate rose-red. Ammonia takes up the oxide of manganese, and, therefore, the oxides of copper and cobalt cannot be separated from it by this alkali. The least trace of cobalt gives the muriatic solution a permanent emerald-green colour. Dr. John's method of purifying manganese would not have succeeded if he had not reduced the oxide of iron to the minimum by precipitating the copper.

There are two oxalates: that ad minimum is pulverulent, of a fine permanent yellow colour, and nearly insoluble in water; that ad maximum is brown or grayish-green, and soluble in water, especially if hot. Both are soluble in a slight excess of the acid; so that if a little red oxide of iron had been present in Dr. John's solution, it would have been separated from the manganese.

Five grammes of carbonate of manganese yielded 5.1 gr. of phosphate, so that the carbonate contains 56 per cent. of oxide ad minimum, and 34.4 of carbonic acid; as the carbonate was found to lose that proportion of its weight by solution in sulphuric acid, of course, there remains 9.6 of water.

Carbonate of manganese yields from 59.5 to 62.5 of brown oxide by calcination; these oxides are of different shades, and some cannot be distinguished by the eye from red oxide of iron.

20 gr. of carbonate of manganese yielded by reduction 8.8 of metal, but it was porous, and contained some unreduced particles. 5 gr. of carbonate melted with 15 of red oxide of iron (which yielded on reduction 70.5 per cent. of cast iron), yielded 12.65 of a white crystalline alloy: another experiment yielded 12.7, but there always remains some part unreduced. As carbonate of manganese contains, therefore, 42 per cent. of metal, or 56 of oxide ad minimum; the oxide itself contains about 75 per cent. of metal, and 25 of oxygen. The oxide yielded by the calcined carbonate consisted of 73.5 to 67 2 per cent. of metal, and from 26.5 to 32.8 of oxygen. The oxide ad minimum acquires 11 per cent. of oxygen by calcination, and the completest oxide obtained, held 91 per cent. of that ad minimum.

Chemical History of Manganese. By Dr. JOHN.—*Journ. des Mines, No.* 135.

This is a continuation of the paper inserted in our last number.

Nitric acid easily forms a colourless solution of manganese, which yields, on evaporation to dryness, black oxide: the solu-

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tion itself resembles that of the oxide. Green oxide, and carbonate of manganese, are both of them dissolved in nitric acid. The solution, being neutral, was crystallized by a very careful evaporation so as not to decompose the nitrate, and sudden cooling; the mass thus obtained was moistened with a little water, heated, then covered up, and left in the temperature of (59° Fahr.) 12° Réaum. Acicular prismatic crystals were found next morning deposited at the bottom. They were white, semi-transparent, striated lengthways, sharp, bitter, and more easily deliquescent than muriate of manganese; they melt at a very low temperature, and are decomposed by heat. Alkohol dissolves them, and paper impregnated with the solution burns with a green flame.

Liquid benzoic acid acts very slowly upon manganese; the water is decomposed by degrees, and then the oxidized metal is taken up. The green oxide and the carbonate are also slowly dissolved; the solutions are colourless, and easily crystallizable into long thin crystals, or in irregular flakes, if the evaporation was hasty. The crystals are colourless, transparent, permanent in the air, astringent, sweetish but afterwards bitter. They are soluble in alkohol, and in 20 times their weight of water at 15° R. (66° Fahr.), but in less of boiling water. The solution crystallizes by cooling; and alkaline prussiates, carbonates, phosphates, and tunstates decompose it. The crystals on distillation yielded a few drops of water, and a quantity of oil which had the colour, consistence, and fine smell of oil of cinnamon, mixed with that of prussic acid, but none of this acid could be discovered. The residuum was dissolved in muriatic acid, and precipitated by carbonate of potash, by which the benzoate was found to contain 24 per cent. of suboxide of manganese, and, of course, 76 of acid and water.

Succinic acid readily dissolves manganese; hydrogen, smelling like assafœtida, is emitted; the solution is first green, but at last, rose-colour: some charcoal is always separated from the metal. The suboxide and carbonate of manganese are easily soluble, the latter effervesces violently. The solution crystallizes very easily in four sided prisms, double pyramids, or tables. The crystals are transparent, colourless when single, but rose-red in groups; they are of a salt sharpish taste, very brilliant and permanent in the air; but become white and opake by heat, and assume the appearance of porcelain at 15° Réaum. They are soluble in 10 times their weight of water, but insoluble in alkohol. By distillation they yield a little water, oil, carbonic acid, and carburetted hydrogen. The residuum, treated as above, showed that the crystals contained 30.27 per cent. of suboxide, and, of course, 69.73 of acid and water.

Acetic acid slowly forms a reddish solution of manganese, which crystallizes very readily. The suboxide and carbonate dissolve in warm acetic acid, but it requires several days to saturate the

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acid with the suboxide. This acetate crystallizes in rhomboidal tables; they are rose-red, transparent, permanent in the air, and have a disagreeable, astringent, metallic taste. They are soluble in alkohol, and 7 parts of water take up 2 of the crystals. They are decomposed by heat, like the succinate, and thus are found to contain 30 per cent. of suboxide, and 70 of water and acid. The solution is decomposed by alkaline carbonates, prussiates, molybdenates, and arseniates, but not by the alkaline borates or oxalates. Pure arsenic acid throws down a white arseniate of manganese.

Chromic acid acts exceedingly slow upon manganese, although at last the water is decomposed and hydrogen is emitted: but it dissolves the green oxide, and also the carbonate, with which last it effervesces. The solution always contains an excess of acid, it is chesnut-brown, and has a rough metallic taste, it cannot be crystallized, and during the evaporation the manganese is oxidized more than before, and falls down in a black pulverulent chromate. By adding water occasionally, the solution parts with almost all the manganese in this form, and chromic acid, nearly pure, is left. The chromate is decomposed by the alkaline carbonates and prussiates. Nitrate of silver throws down a scarlet precipitate or chromate of silver holding a little manganese, and nitrate of manganese is left in the liquid.

It required a digestion of several weeks to form a combination of tunstic acid, with manganese, its green oxide, or the carbonate, but at last it formed a white granular powder. Tunstate of potash throws down a white tasteless precipitate from the solutions of manganese. This tunstate is insoluble in water, not altered by the air, not fusible by the blowpipe or charcoal, but turns first yellow, and then brown.

Arsenic acid dissolves manganese, and the arseniate separates in a gelatinous form. It also dissolves the green oxide and carbonate; these solutions are always acid; on trying to saturate the acid, they become gelatinous, and deposit a neutral arseniate insoluble in water. A few drops of dilute sulphuric acid added to the jelly reduce it to a clear liquid, that yields radiated bundles of crystals by evaporation, which probably contain both the acids united with the manganese.

On the Oxidizement of Manganese.

It is extremely difficult to determine the proportion of oxygen in the various oxides of manganese, because the suboxides absorb oxygen from the atmosphere, and the others emit oxygen at a high temperature.

There appear to be three oxides of manganese that contain a fixed quantity of oxygen, viz. the green, brown, and black; but there exist several passages of these oxides, as the greenish-gray, arising from the disoxygenizement of the green oxide, and the white arising from the last. Although the solutions of man-

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ganese are red, no oxide of that colour has hitherto been discovered. The stones that contain manganese are also frequently red, but this may arise from other causes.

Manganese decomposes water, at the usual temperature, with astonishing rapidity. 80 grains of manganese placed in distilled water, yielded 24 oz. measures of hydrogen, which probably contained some of the metal, as it had a peculiar smell, and burned with a green flame, and there was left 92 gr. of a grayish-green oxide, which, of course, contained 86.97 per cent. of metal, and 13.03 of oxygen. As the carbonate contained 48.6 per cent. of metal, and left, on being ignited in a retort, 55.84 of greenish oxide; this oxide contained 87.04 of metal and 12.96 of oxygen: so that 13 per cent. of oxygen may be looked upon as a medium.

The 92 gr. of grayish-green oxide, being exposed to the air until it changed to a pure deep brown, and then heated for a moment to expel the moisture, was augmented 8 gr. in weight, so that this oxide contained 90 per cent. of metal, and 20 of oxygen. The same result was obtained by leaving manganese exposed for some days to the air, and expelling the moisture by heat. This brown oxide absorbs some more oxygen by being long exposed to the air, but extremely slow.

One hundred gr. of manganese were dissolved in nitric acid, and the solution distilled until it ceased to emit vapours, it left 140 gr. of porous, brilliant black oxide, which did not contain, on trial, any traces of nitrate of manganese, and yielded pure oxygen by heat. Of course, black oxide contains 71.33 of metal and 28.67 of oxygen.

On a new Kind of Pitch Iron Ore, or native Sulphate of Iron with Excess of Base. By Mr. Gillet Laumont.—*Journ. des Mines, No.* 135.

Pitch iron ore has been confounded with Freyberg black blende, and suboxide of uran, and lately Werner has given this name to the phosphated manganese of Hauy.

This mineral was first noticed by Karsten, in Ferber's collection, as brought from the Kust-bescheerung mine near Freyberg; but a similar substance had been previously found in the lead-mines of Huelgoat, in Bretagne.

Its specific gravity is 2.144. It is of a deep yellow colour, forming a passage between that of olivine and idocrase or vesuvian. It has a resinous aspect with a conchoidal, unequal, brilliant fracture. It is harder than gypsum, and scratches calcareous spar, but is also deeply scratched by the spar. It is extremely brittle; the scraping is yellow, that from Freyberg is rather the deepest. It puffs up before the blowpipe, splits, acquires the colour of

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gamboge in the lump, and melts into black scoriæ, which adhere to the support; these scoriæ are attractible by the magnet: the variety from Freyberg swells the most, melts quickest, and adheres firmer to the platina forceps.

Klaproth found it to contain 67 per cent. of oxide of iron, 8 of dry sulphuric acid, and 25 of water.

When the variety of Huelgoat was recently collected, it had an acid styptic taste, and threw down a white precipitate from muriate of barytes. The presence of phosphoric acid was suspected because it changed the flame of the blowpipe green, and gave a white precipitate with lime-water; especially as the Huelgoat lead ore produces a green flame when heated, and will yield from 4 to 5 per cent. of phosphorus with great ease.

On some petrified Serpents. By Mr. Stifft.—*Journ. des Mines, No.* 135.

About a league and a half from Dillenbourg (which lies 12 leagues N. W. from Frankfort) is a chain of hills composed of common graywakke, graywakke-slate, clay slate, and transition limestone, covered with beds of green stone and transition trap.

In these hills, about half a league N. E. of Herbornsseelbach, in a quarry of graywakke, serpents have been found petrified in a bed of clay, which appears to be decomposed graywakke slate, and lies under a bed of graywakke, between that and a bed of graywakke slate.

The serpents are flattened underneath, but their back is raised, and engaged in the upper bed of graywakke. One of them is about 16 or 17 in. long, and 7 or 8 lines diameter; it forms several turns, but has neither head nor tail. Another has a head, in which, however, neither eyes nor mouth can be discovered. A great number have been found, but in a very imperfect state.

Observations.—As no petrifactions, but those of zoophytes, have hitherto been found in the transition rocks, it is probable that these are not the remains of serpents, but of some unknown zoophyte.

Experiments on rectified acetous Acid. By Philip Anthony Steinacher.—*Journ. de Phys. Dec.* 1807.

67968 parts of acetic acid obtained from verdigris, yielded on rectification, *a*, 18432 of acid, marking 1° by Vincent's hydrometer for acids, at 12° R:, *b*, 9216 of acid at 4°; *c*, 9216 of acid, at 8°; *d*, 9216 of acid at 9°; *e*, 11520 of acid at 9½°; *f*, 6912

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of acid at 10° which did not crystallize at 7° R. but at 0°; *g*, 2304 of acid, whose specific grav. was $\frac{889}{450}=1.860$, which crystallized at 7°, and melted at 10°, R. There remained 432 parts of a thick viscous matter consisting of oxide of copper and some acid.

These portions were clear, but had a citrine tinge; the portions *c*, *d*, *e*, were mixed together and distilled still more slowly: they yielded *h*, 9216 of acid, marking 8° hydr.; its sp. grav. was $\frac{772}{450}=1.480$; the liquor now in the retort marked 9° hydr. and was scarcely coloured: *i*, 9216 of acid at 9°, sp. grav. $\frac{777}{450}=1.610$; the liquor now in the retort marked 9°, and was of a fine citrine colour: *k*, 8064 of acid at 10°, sp. grav. $\frac{778}{450}=1.620$. The residuum weighed 3456, was of a deep yellow, and appeared loaded with oil; it marked 8° hydr. and crystallized at 7° R. although the acid *k* did not crystallize until cooled to the freezing point.

The last residuum was again distilled, until there remained only 69 of a brown-black, oily, empyreumatic liquid, which was rendered colourless by three times its bulk of oxymuriatic acid: the pellicles thus formed, were soluble, after washing with distilled water, in alkohol rectified to 30° hydr. and formed a yellow solution. The acid that came over was perfectly transparent and colourless: even by a perpendicular light, it did not crystallize at 7° R.; its sp. grav. was 1.620.

The portion *f* was distilled, it yielded 4608 of acid, sp. grav. 1.620, as clear as rock-crystal, and there remained 2304 of a deep yellow residuum, crystallizing at 7° R. The uncrystallized liquid being drained off, was of a deeper colour than before; and the crystals being melted, formed only an extremely pale citrine liquor. So that the attraction of the acid for oil divides the liquid into two portions; one in which the oil predominates and that does not crystallize; the other, in which the acid predominates and is crystallizable.

The uncrystallized portion did not yield any more crystals at 7°, although its sp. grav. was $\frac{776}{450}=1.570$. The crystallized portion did not melt at 14° R. and after being melted, it again crystallized in a mass at 10°, absorbing air, and emitting heat. Its specific gravity after fusion was only $\frac{770}{450}=1.440$, and yet it was at once the most inflammable, and the most acid. For it takes fire, and burns with a blue flame on a light being brought near it when heated to the boiling point; whereas the acid, whose sp. grav. is 1.620, does not take fire until it boils violently. The inflammability of the uncrystallizable portion cannot be well ascertained on account of the oil united with it. And again, 4 parts of the acid, sp. grav. 1.440, diluted with 72 of water, saturated 16 of dry crystallized carbonate of potash, whereas the same quantity of acid, sp. grav. 1.620, only saturated 12 parts.

Observations.—The specific gravity of the various portions of

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the acid, are expressed both in vulgar and decimal fractions; but it is certain that one of the statements must be wrong, because the expressions are not equivalent, as will be evident to most English schoolboys. This is, however, such a common fault in those French chemists who have been bred to pharmacy, that their statements are always to be suspected when expressed decimally.

From these experiments it appears that acetic acid does not follow the general law of other acids, in respect to its being stronger in proportion to its superior heaviness, but the contrary. And therefore it is necessary to ascertain the quantity of potash that any specimen of the acid will saturate, in order to judge of its strength.

On the Use of Sheep's Dung, in dying Cotton of a Turkey-red Colour. By J. B. VITALIS.—*Journ. de Phys. Feb.* 1808.

THE manufactories of Rouen are famous for the excellency of the Turkey-red colour, and those other colours that depend upon it, which they give to their cotton goods: the first preparation given to these goods consists in putting them into a bath of sheep's dung.

Pileur d'Apligny, and Felix, suppose that sheep's dung, and the intestinal liquor of sheep, are of no use in fixing the colour, but that they contain a great quantity of ready-formed volatile alkali, which enlivens the red colour given by the madder. As to the ideas of the workmen themselves, it is quite sufficient to recite but one, that the dung is only useful on account of its containing the ground-down particles of vegetables which serve to divide the oil, that is always used along with it.

Fourcroy remarks, that the mucus which lines all the internal parts of the body, contains a kind of gelatinous albumen, not easily dried, being rather inclined to deliquescence. All author who have spoken of the intestinal liquor, have represented it as a viscous, thick, glairy or mucous liquid, coagulable by fire and acids, and also very soluble in alkalies. There can be no doubt but that this liquid is contained in great abundance in the dung, and produces the effects. It is evident also, that ready-formed volatile alkali is not present in the bath, while fresh.

On distilling 61.19 grammes of fresh sheep's dung, it yielded 0.58 gr. of gaseous fluids, 3.91 of oil, 7.8 of charcoal and phosphate of lime, and 48 8 of a coloured turbid liquor, which slightly reddened tincture of litmus, and at the same time rendered syrup of violets green, but no traces of ammonia could be discovered; so that the opinion of Pileur d'Apligny is totally unfounded on facts. Even if ammonia was present, it would certainly be dissipated by the various operations and dryings even in stoves, which the cotton undergoes. Still less can it be said that ana-

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monia would enliven the colour given by madder, as it is well known that this can only be effected by forming, with white soap and muriate of tin, a metallic soap holding oxide of tin in solution by soda.

The real active principle of sheep's dung is an albumino-gelatinous matter: this is evident from the manner in which it is used. The dung is macerated in a solution of soda at only 4° hydr. The macerated liquor is then run through a sieve and mixed with more solution of soda at 2° hydr. and some thick or mucilaginous olive-oil, by which means a liquid animal soap is formed, so that the cotton being impregnated therewith, is made to approach to the nature of animal substances, and thus its attraction for the colouring matter is increased.

The truth of this theory is confirmed by actual experiments. Fresh sheep's dung was macerated for four or five days in a ley of soda at 4° hydr. and a reddish-brown solution was obtained, which yielded an abundant precipitate, which settled at length at the bottom of the vessel. This precipitate, previously washed with cold water, was boiled in distilled water for an hour; the reddish-yellow liquor being decanted, yielded a precipitate with tannin, so that it evidently contained gelatine, while the quantity of coagulated albumen in the liquor, showed that albumen was the predominant ingredient, and about three times as abundant as gelatine.

A trial was made to supply the place of the dung baths, by means of an alkaline solution of white of egg, or albumen, which perfectly succeeded, as the colours were much more fixed than when the use of sheep's dung was neglected.

Observations.—There is, perhaps, no chemical art usually practised in which so many strange processes are used as in dying. In reviewing the prescriptions one is often led to wonder how the original inventor was induced to adopt either the ingredients or the manipulation. The subject of the above paper is evidently of this class; for who would have thought of using sheep's dung to impregnate cotton with albumen?

The simplification of the complex processes frequently used in the arts without any necessity, and the discovery of the principles on which they depend, are objects of the first consequence to the real lover of chemistry, to him who wishes to blend the useful with the brilliant. We are glad to find the French chemists are beginning to proceed in this career; the excellent papers of Berthier, on smelting iron ore, show the value of these researches, and we hope that Mr. Vitalis will go on in his proposed examination of the processes used in the dye-houses of Rouen.

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Analysis of a slaty Hornblende found in the West of France By Mr. CHE'VREUL.—*Journ. de Phys. Feb.* 1808.

THIS hornblende is blackish-green passing into blueish-green; sometimes it is slaty in the large, and splintery in the small. It is hard to melt by the blowpipe, but at last it yields a black glass. It is very common about Nantes, and from thence to L'Orient, as also in Britany as far as Brest.

Ten grammes of this hornblende boiled in sulphuric acid at 30° hydr. left 4.7 of insoluble matter, from whence 4.45 of pure silica were obtained by fusion with potash and elixiviation with muriatic acid.

Hornblende distilled with sulphuric acid yielded a considerable quantity of sulphurous acid gas, which shows that the oxides contained in the hornblende are at a minimum of oxidizement. When distilled with nitre, the emitted gases did not precipitate lime-water. When heated with potash it formed a yellow and green paste, which yielded a fine green ley, from which, yellow flakes of oxide of manganese separated by boiling, and the ley became gold-yellow, which indicates the presence of chromic acid.

The hornblende was found to contain 45 per cent. of silica, 8 of alumine, 10.84 of lime, 6 of magnesia, 22 of oxide of iron with some of manganese, 1 of oxide of chrome, and 0.5 of potash with some traces of another substance which was probably a hydrosulphuret of zinc; 6.66 were lost in the analysis owing partly to water. This analysis approaches very nearly to Laugier's analysis of the amphibole of the Cape de Gates. The chrome that this hornblende contains, is probably combined with the iron; especially as chromate of iron being pulverized and put into dilute sulphuric acid, causes it in the course of a few months to become green, it being kept in the dark. If the chrome was acidified, it is not probable that the diluted sulphuric acid would be able to reduce it to the minimum of oxygenizement.

NATURAL PHILOSOPHY, ARTS, AND MANUFACTURES.

Of the Solids of greatest Attraction, or those which, among all the Solids that have certain Properties, attract with the greatest Force in a given Direction. By JOHN PLAYFAIR, *F. R. S. Lon. and Edin.—Edinburgh Trans. Vol. VI. Part II.*

PROFESSOR Playfair divides the subject of this inquiry into a number of distinct propositions, and considers each separately; we shall, therefore, follow him through their regular order, and present our readers with his principal results.

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1. "To find the solid into which a mass of homogeneous matter must be formed, in order to attract a particle given in position, with the greatest force possible, in a given direction." The figure may easily be conceived, in which A denotes the particle given in position; AB the direction in which it is attracted; and ACBH a section of the required solid made by a plane passing through the line of attraction AB. Mr. Playfair then reasons thus: since the attraction of the solid is a maximum, any small variation in its figure, provided the quantity of matter remain the same, will not change the attraction in the direction AB. The curve ACB is, therefore, the locus of all the points in which a body being placed, will attract the particle A with an equal force in the direction AB. This condition is deemed sufficient to determine the nature of the curve. For a perpendicular CE being let fall upon AB, and A,C joined; it is soon found that all the sections of the required solid, made by planes passing through AB, have this property; viz. $AC^3 = AB^2 \times AE$. The solid generated by the revolution of the plane ACB about AB as an axis, is styled the *solid of greatest attraction*; and ACB the *curve of equal attraction*.

2. "To find the equation between the co-ordinates of ACB, the curve of equal attraction." The same figure being employed; make $AB=a$, $AE=x$, $EC=y$. Then from the first problem $a^2x=(x^2+y^2)^{\frac{3}{2}}$, or $a^4x^2=(x^2+y^2)^3$; which is an equation to a line of the 6th order. To obtain y in terms of x, $x^2+y^2=a^{\frac{4}{3}}x^{\frac{2}{3}}$; hence $y=x^{\frac{1}{3}}\sqrt{a^{\frac{4}{3}}-x^{\frac{4}{3}}}$. The results which this equation presents are obvious.

3. "The curve may easily be constructed without having recourse to the value of y just obtained." If $AC=z$, and the variable angle BAC (C being any point in the curve as before) $=\phi$; we have $z=a\sqrt{\cos.\ \phi}=a\sqrt{\frac{AG}{AB}}=\sqrt{AB\times AG}$; where G denotes a point upon AB determined by a perpendicular let fall from the point where a circle, described with A as a centre, and AB as a radius, meets AC produced in F. "Therefore, if from the centre A, with the distance AB, a circle BFH be described, and if a circle be also described on the diameter AB, as AKB, then drawing any line AF from it, meeting the circle BFH in F, and from F letting fall FG perpendicular on AB, intersecting the semicircle AKB in K; if AK be joined, and AC made equal to AK, the point C is in the curve."

4. "To find the area of the curve ACB." This is found equal to $\frac{1}{4}AB^2$; or the area of the curve on both sides of AB is equal to AB^2. When y is a maximum, $x=a\div\sqrt[4]{27}$: and therefore, if the value of y be denoted by b, we shall have $a:b::\sqrt[4]{27}:\sqrt{2}$; or as 11 : 7 nearly. The radius of curvature at A is infinite; or the solid of greatest attraction, at that extremity of

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its axis where the particle is situated, approaches nearer to a plane than the superficies of any circle can do, however great its radius. The radius of curvature at the opposite end of the axis, or at B, is $\frac{3}{2}$AB.

5. "To find the force with which the solid above defined attracts the particle A in the direction AB." The whole attraction of the solid is equal to $\frac{4\pi}{5}a$. Therefore this attraction is to that of a sphere on the same axis, as $\frac{4\pi}{5}a$ to $\frac{2\pi}{3}a$; or as 6 to 5.

6. "To find the content of the solid." This content is found to be to that of a sphere on the same axis as $\frac{4}{15}$ to $\frac{1}{6}$; or as 8 to 5.

7. The attraction of this solid when compared to that of a sphere of equal magnitude, is found to be as 3 to $\sqrt[3]{25}$; or nearly in the ratio of 81 to 79.

In the preceding investigations the particle upon which the attractive force is supposed to operate, has been conceived to be in contact with the solid; and Mr. Playfair now considers the problem when there is a given distance between them.

8. The solid being supposed to be on one side of a plane, and the particle at a given distance on the opposite side, it is found that the required solid is a segment of the *solid of greatest attraction*, having B for its vertex, and a circular base terminated by the above plane perpendicular to its axis.

9. Conceive AHB to be a section of the solid made by a plane passing through its axis AB. Let G be any point in the superficies of the solid, GF a perpendicular from G to the plane AHB, and FE a perpendicular from F on the axis AB. Then put the three co-ordinates, AE$=x$, EF$=z$, FG$=v$, and let AB$=a$, EH$=y$; then $(x^2+z^2+v^2)^3=a^4x^2$ will be the equation to the superficies of the solid of greatest attraction.

10. Hitherto the law of the attracting force has been supposed to vary inversely as the square of the distance; but Mr. P. next considers the subject when this law varies inversely as the mth power of the distance. Putting AE $= x$, and EC $= y$, in the first conceived figure, he finds $y^2 = a^{\frac{2m}{m+1}}x^{\frac{2}{m+1}} - x^2$. If $m = 1$, or the attracting force vary inversely as the distance, the equation becomes $y^2 = ax - x^2$, and the solid of greatest attraction would be a sphere. If $m = 3$, or the force be inversely as the cube of the distance, the equation is $y^2 = a^{\frac{1}{2}}x^{\frac{1}{2}} - x^2$; which belongs to a line of the 4th order. When $m = 4$, the equation is $y^2 = a^{\frac{8}{5}}x^{\frac{2}{5}} - x^2$; belonging to a line of the 10th order. When m is any even number the order of the curve is $\overline{m+1} \times 2$; but if m be an odd number it is simply $m + 1$.

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11. In this article the Professor observes, "In the same manner that the solid of greatest attraction has been found, may a great *class* of similar problems be resolved. Whenever the property that is to exist in its greatest or least degree, belongs to all the points of a plane figure, or to all the points of a solid, given in magnitude, the question is reduced to the determination of the locus of a certain equation, just as in the preceding example." He then gives an instance in finding a solid of a given magnitude, such, that from all the points in it, straight lines being drawn to any assigned number of given points, the sum of the squares of all the lines so drawn shall be a *minimum*. This he finds to be a sphere, having for its centre the centre of gravity of the given points. He also proves generally, "if x, y, and z, are three rectangular co-ordinates that determine the position of any point of a solid given in magnitude, and if the value of a certain function Q, of x, y, and z, be computed for each point of the solid, and if the sum of all these values of Q added together, be a maximum or a minimum, the solid is bounded by a superficies in which the function Q is every where of the same magnitude."

12. The preceding theorems relate to such solids of a given content, as have the greatest attraction in a given direction. The next inquiry is, which body of a given kind, and of a given solid content, has the greatest attractive power in a certain direction. The cone of greatest attraction is that which has half the angle at the vertex 62° 46', and the radius of its base nearly double its altitude. The attraction of such a cone is about $\frac{4}{5}$ of that of a sphere of equal solidity.

13. When the particle is placed on one extremity of the axis of a cylinder, and the attraction of the cylinder is a maximum, the diameter of its base is to its altitude as $9-\sqrt{17}$ to 4, or as 5 to 4 nearly. And the attraction of this cylinder does not exceed that of a sphere of equal solidity by more than one 183d part.

14. The whole attraction of an elementary column, standing perpendicularly on its basis, and erected on a point in the plane of that basis, "is equal to the area of the base divided by the distance, and multiplied by the sine of the angle of elevation of the column." Thus if m^2 express the area, r the distance, and ϕ the angle subtended by the column, the expression for the attraction is $\frac{m^2}{r}$ sine ϕ. "In this investigation, m^2 is supposed an infinitesimal; but if it be of a finite magnitude, provided it be small, this theorem will afford a sufficient approximation to the attraction of the column, supposing the distance to be measured from the centre of gravity of the base, and the angle ϕ to be that which is subtended by the axis of the column, or by its perpendicular height above the base."

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15. The attraction of a semi-cylinder upon a particle situated at the centre of the semicircle which constitutes its base, is expressed by the formula $2\,a\,L\,\frac{r+\sqrt{a^2+r^2}}{a}$; where a denotes the altitude of the cylinder, r its radius, and L the hyp. log.

16. When a semi-cylinder given in magnitude attracts a particle situated at the centre of its base with the greatest force possible, the radius of the base of the half-cylinder is to its altitude nearly as 216 to 125.

17. The attraction upon a particle placed at the pole of an oblate spheroid of given magnitude is not a maximum, until its oblateness vanish, and the spheroid become a sphere; the attraction of which is expressed by the formula $m\sqrt[3]{\frac{16\,\pi^2}{9}}$; where m is the cube root of the solidity.

18. For the force with which a parallelopiped attracts a particle of matter, in a direction perpendicular to any of its sides, Mr. Playfair gives this rule: "Multiply the sine of the greatest elevation, into the sine of the greatest azimuth of the solid; the arch of which this is the sine, multiplied into the thickness of the solid, is equal to its attraction in the direction of the perpendicular from the point attracted."

19. An isosceles pyramid, with a square base, exerts a maximum attraction upon a particle at its vertex, when the inclination of the opposite planes to one another forms an angle of 153°.

20. In this article, Mr. P. shows generally, that, "if a particle A gravitate to a rectangular plane, or to a solid indefinitely thin, contained between two parallel rectangular planes, its gravitation, in the line perpendicular to these planes, will be equal to the thickness of the solid, multiplied into the area of the spherical quadrilateral subtended by either of those planes at the centre A." This general proposition may also be extended to all planes, by whatever figures they are bounded.

21. In this proposition, it is proved "that, whatever be the figure of any body, its attraction will decrease in a ratio that approaches continually nearer to the inverse ratio of the squares of the distances, as the distances themselves are greater; and with which it may be said to coincide when the distances are infinitely great."

22. The object of this proposition is to find the attraction of a parallelopiped, of any given dimensions, in the direction perpendicular to its sides; but our want of diagrams and the complexness of the result prevent us from transcribing it in this place.

Observations.—The problem respecting the solid of the greatest attraction appears to have been first solved by Silvabelle: his

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paper was printed in the *Mémoires des Savans Etrangers*, in 1750. About ten years since it was investigated by some of our own mathematicians in one of the Diaries: but Professor Playfair has extended his researches considerably beyond those of his predecessors. At the commencement of this essay we are informed that its author was led to the investigations it contains, by considering the experiments and researches of *Dr. Maskelyne* and *Dr. Hutton* on the attraction of mountains, and those of Mr. Cavendish, on leaden balls. In reflecting on the labours of these philosophers, it occurred to Mr. Playfair to inquire what form a given mass of matter ought to have, that it might attract a particle in a certain direction with the greatest force possible. This inquiry belongs to a class of problems of considerable difficulty, and in the resolution of which the *calculus variationum* has generally been employed. Professor Playfair's ingenuity, however, soon discovered a much simpler method of solution; which he has successfully applied to both this and the other similar problems treated in this paper. His investigations unite a high degree of elegance and simplicity, and lead to results that are equally interesting and new. There does not appear to be a single step in the whole of the processes that constitute this essay of 37 quarto pages, which may not be easily understood by any person who possesses a common acquaintance with the fluxionary calculus; and although we have been as brief as possible in giving the Professor's principal results, we hope that this brevity will not prevent any of them from being fully understood by our mathematical readers: at the same time, we think ourselves warranted to observe, that those who have an opportunity of perusing the original, will find themselves amply repaid for their trouble.

By comparing the 12th and 19th propositions, it will readily be conceived that a considerable difference subsists between the angles at the vertices of the cone and isosceles pyramid of greatest attraction; Mr. P. states the former to be 125° 32′, and the latter 153°: a difference of 27° 28′. This difference between the vertical angles is not only much greater than we were prepared to expect, but also of a contrary nature; and a few moments' reflection on the properties of the two solids convinced us that the angle of the pyramid should be a little less than that of the cone; hence, the natural inference was, that Mr. P. had fallen into some inaccuracy in one of these investigations. Near the conclusion of the 19th proposition, he determines that the arc $\varphi = 48° 40\frac{2}{3}'$; and adds, "since sin. φ = sin. n^2, sin. $n = \sqrt{}$ sin. φ, so that $n = 76° 30'$, and $2n$, or the whole angle of the pyramid = 153°." Such of our readers, however, as will take the trouble of performing the arithmetical operation denoted by $\sqrt{}$ sin. φ, will easily satisfy themselves that $n = 60° 4'$ nearly, and not 76° 30′; and consequently, that the angle of the

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pyramid is 120° 8′, instead of 153°. How a reflection similar to that which induced us to examine Mr. Playfair's numerical computations, should have escaped the penetration of that learned author, is not easily accounted for; it, however, strongly evinces the constant circumspection which ought to be maintained in all investigations of a similar nature.

Mr. Playfair also observes, that what he has offered in this paper, is an investigation of some theorems employed in obtaining conclusions from a survey of the rocks which compose the mountain of *Schehallien*, made by Lord Webb Seymour and himself, with the view of forming a tolerable estimate of their specific gravity, and thence correcting the conclusions deduced from Dr. Maskelyne's observations concerning the mean density of the earth. If the present disquisition be a fair specimen of the manner in which these researches have been conducted, we hope the Professor will not suffer any more time to elapse than is absolutely necessary to complete the undertaking before he lay an account of this survey before the Society which caused the original experiments to be instituted: and without, in the least, impeaching what has been done on the subject, we conceive that we are authorized to augur favourably respecting the communication whenever it may be made.

New Series for the Quadrature of the Conic Sections, and the Computation of Logarithms. By WILLIAM WALLACE, *F. R. S. Edin.—Edin. Trans. Vol. VI. Part II.*

FROM the introductory part of this paper, we learn, that the object of Mr. Wallace is to investigate series for the rectification of circular arcs, the quadrature of the conic sections, and the computation of logarithms, without employing either the fluxional or any other equivalent calculus. He proceeds to these different investigations in their order; and for obtaining a series applicable to the rectification of circular arcs, he assumes, from the arithmetic of sines, the well-known formula,

$$\frac{1}{\text{tan. A.}} = \frac{1}{2 \text{ tan. } \frac{1}{2} \text{ A.}} - \frac{1}{2} \text{ tan. } \frac{1}{2} \text{ A};$$

where A denotes any arch of a circle, the radius of which is unity. From this formula, by supposing P to represent the perimeter of a figure constructed by dividing the arch a into 2^{n-1} equal parts, drawing tangents to the points of division and the extremities of the arch, and producing them till they meet each other, he shows, that

$$\frac{1}{P} = \frac{1}{\text{tan. } a} + \frac{1}{2} \text{ tan. } \frac{1}{2}a + \frac{1}{4} \text{ tan. } \frac{1}{4}a + \frac{1}{8} \text{ tan. } \frac{1}{8}a + \frac{1}{16} \text{ tan. } \frac{1}{16}a \ldots + \frac{1}{2^n} \text{ tan. } \frac{a}{2^n};$$

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and this is true, whatever be the number of terms in the series

$$\tfrac{1}{2}\tan.\tfrac{1}{2}a + \tfrac{1}{4}\tan.\tfrac{1}{4}a \ldots\ldots + \frac{1}{2^n}\tan.\frac{a}{2^n}.$$

Then by an application of a principle admitted in the Elements of Geometry; viz. that an arch being divided, and a polygon described about it, the perimeter of the polygon will continually approach to the circular arch, and will, at last, differ from it by less than any given quantity; and conceiving this division to go on *ad infinitum*, and substituting the arch instead of the perimeter, to which it will at last become equal, the above series is changed into

$$\frac{1}{a} = \frac{1}{\tan.\,a} + \tfrac{1}{2}\tan.\tfrac{1}{2}a + \tfrac{1}{4}\tan.\tfrac{1}{4}a + \tfrac{1}{8}\tan.\tfrac{1}{8}a + \tfrac{1}{16}\tan.\tfrac{1}{16}a + \&c.;$$

and this is Mr. Wallace's first series for the rectification of the circle. This, however, expresses only the reciprocal of the arch, but affords a ready means of finding the arch itself.

Mr. Wallace's second series for the same purpose, when reduced to the most convenient form, is

$$a^{\frac{1}{2}} = \begin{cases} \frac{1}{4}\,\dfrac{1+\cos.\,a}{1-\cos.\,a} + \frac{1}{6} \\ -\left(\dfrac{1}{4^2}\,\dfrac{1-\cos.\frac{1}{2}a}{1+\cos.\frac{1}{4}a} + \dfrac{1}{4^3}\,\dfrac{1-\cos.\frac{1}{4}a}{1+\cos.\frac{1}{8}a} + \dfrac{1}{4^4}\,\dfrac{1-\cos.\frac{1}{8}a}{1+\cos.\frac{1}{16}a}\right. \\ \left. + \dfrac{1}{4^5}\,\dfrac{1-\cos.\frac{1}{16}a}{1+\cos.\frac{1}{16}a} +, \&c.\right) \end{cases}$$

His third series for this rectification is

$$a^{\frac{1}{3}} = \begin{cases} \dfrac{\sec.^2 a}{\tan.^3 a} + \frac{1}{8}\tan.\frac{1}{2}a\sec.^2\frac{1}{2}a + \frac{1}{8^2}\tan.\frac{1}{4}a\sec.^2\frac{1}{4}a \\ + \dfrac{1}{8^3}\tan.\frac{1}{8}a\sec.^2\frac{1}{8}a +, \&c. \end{cases}$$

This series is evidently of a simple form, and of considerable convergency; each term being less than the 16th part of that which precedes it.

Mr. W.'s fourth series is

$$\frac{1}{a^4} = \frac{1}{3.16^2}\,\frac{13-\cos.\,a+12\cos.\frac{1}{2}a}{3+\cos.\,a-4\cos.\frac{1}{2}a} + \frac{7}{8.8.9.10}$$
$$-\left\{\frac{1}{3.16^3}\,\frac{13-\cos.\frac{1}{2}a-12\cos.\frac{1}{4}a}{3+\cos.\frac{1}{2}a+4\cos.\frac{1}{4}a}\right.$$
$$+\frac{1}{3.16^4}\,\frac{13-\cos.\frac{1}{4}a-12\cos.\frac{1}{8}a}{3+\cos.\frac{1}{4}a+4\cos.\frac{1}{8}a}$$
$$\left.+\frac{1}{3.16^5}\,\frac{13-\cos.\frac{1}{8}a-12\cos.\frac{1}{16}a}{3+\cos.\frac{1}{8}a+4\cos.\frac{1}{16}a} +, \&c.\right\}.$$

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The rate of convergence in this series is very quick, each term being less than $\frac{1}{64}$th of that which precedes it.

As the formula which would result from an investigation of this nature for the parabola must necessarily be the same with those found by any other method, Mr. W. does not treat of this figure. And also as the quadratures of the ellipse and any hyperbola may be deduced from those of the circle and equilateral hyperbola, he only treats of series relative to the latter two. He, therefore, proceeds next to the investigation of analogous series for the equilateral hyperbola. These are obtained by a process similar to that used in the former part of this paper, from the correspondence between tan. $S = \frac{\text{ord. } S}{\text{ab. } S}$, and tan. $A = \frac{\text{sin. } A}{\text{cos. } A}$; where tan. S denotes the tangent drawn from the vertex of the equilateral hyperbola to meet the line which joins the centre and the extremity of the semi-ordinate; while ab. S and ord. S represent the corresponding abscissa and ordinate. The first series for the hyperbola is

$$\frac{1}{s} = \frac{2\,\text{ab. } S}{\text{ord. } S} - \left\{ \frac{P^{\frac{1}{2}} - 1}{P^{\frac{1}{2}} + 1} + \tfrac{1}{3}\,\frac{P^{\frac{1}{4}} - 1}{P^{\frac{1}{4}} + 1} + \tfrac{1}{5}\,\frac{P^{\frac{1}{8}} - 1}{P^{\frac{1}{8}} + 1} + \text{, \&c.} \right\};$$

where s is substituted for S, and $P = \frac{\text{ab. } S + \text{ord. } S}{\text{ab. } S - \text{ord. } s}$. A second series is also investigated in a similar manner, and the result found analogous to the second series above obtained for the circle.

In the third part of this paper similar series for the computation of logarithms are obtained from the equation $x = \frac{y}{m} + \log. b$; and when $b = 1$, this equation becomes $x = y \div m$; where x denotes any number or term of the series, y the exponent of that power of the common ratio of the geometrical series which is equal to x, and m the exponent of that power of this ratio which is equal to b: The first series obtained from this equation is

$$\frac{1}{\log. x} = \tfrac{1}{2}\,\frac{x+1}{x-1} - \left(\tfrac{1}{4}\,\frac{x^{\frac{1}{2}} - 1}{x^{\frac{1}{2}} - 1} + \tfrac{1}{6}\,\frac{x^{\frac{1}{4}} - 1}{x^{\frac{1}{4}} - 1} + \tfrac{1}{16}\,\frac{x^{\frac{1}{8}} - 1}{x^{\frac{1}{8}} + 1} \right.$$

$$\text{... +, \&c.} \Big)$$

Mr. Wallace likewise obtains other series for the same purpose; and then concludes with some examples of their application.

Observations.—The quadrature of curves, or the finding of a rectilineal space equal to a proposed curvilineal one, constitutes

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a part of the sublime or higher geometry, and has, in a greater or less degree, engaged the attention, and exercised the abilities, of many celebrated mathematicians, from the very infancy to the present improved state of that science. As the circle is the simplest, as well as the most useful, of curves, it has not only occupied the earliest, but also the chief part of these labours. This quadrature has been principally undertaken by two distinct classes of mathematicians: and as the learning and ingenuity of the one can hardly fail of instructing several of our readers; while the singular flights of the other, who, like the unfortunate Sisyphus, have been perpetually doomed to roll up a heavy burden without ever attaining its destination, will afford them some amusement, we shall endeavour to present them with a summary of these attempts; taking each class separately.

The circumferences of circles are to each other as their diameters or radii: and as the areas of circles are proportionate to the rectangles of their radii and circumferences, the quadrature of the circle would be effected by the rectification of its circumference; consequently, if the length of the circumference could be accurately found, the true area would also be known. It is said, that Anaxagoras, who was born at Clazomene in Ionia, about the 70th Olympiad, endeavoured to ascertain this length while imprisoned at Athens; but with what degree of success we are not enabled to say, as the result of his inquiries has not been handed down to us. The problem appears to have attained great celebrity in the time of Aristophanes, the renowned comic poet of Athens, who flourished about four centuries before the Christian era, and who, in order to ridicule the celebrated Meto, introduces him, in one of his comedies, as promising to square the circle. It is supposed by many, that Hippocrates, of Chios, who, from being a shipwrecked merchant, commenced geometer, was occupied with this problem when he discovered the quadrature of his *lunula*, we believe the first curves that were squared. Proclus, however, expressly says, that this was not the discovery of Hippocrates, but of Œnopidas, of the same place. The chief object of the author of these lunula appears to have been to show, that if the line described on the side of an inscribed hexagon could be made equal to a rectilineal space, the quadrature could be thence deduced. The circle was also attempted by Euclid, and with what success is well known. Archimedes appears to have taken up geometry where Euclid left it, and is the first who squared the parabola. Before his time geometry had been enriched with more difficult discoveries than the proportion stated at the beginning of this paragraph, yet this truth is first found in the works of Archimedes. This celebrated geometrician, not being able to determine, with geometrical precision, the proportion which subsisted between the diameter and the circumference, had recourse to approximation, and found, by calculating the nu-

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meral value of the inscribed and circumscribed regular polygons of 96 sides, that this ratio is nearly that of 7 to 22. He also computed the area of the circle, and found that it is to the square of the diameter as 11 to 14; and determined the relation between the circle and the ellipse. He likewise squared the parabola, but attempted the hyperbola without success. Several others among the ancient geometricians also engaged in the solution of this interesting problem, but their exact approximations have not reached us.

The first among modern geometricians, who made any valuable additions to what the ancients had transmitted to us respecting the quadrature of the circle, was Peter Metius, a native of the Netherlands, who flourished about the end of the 16th century. While he was employed in refuting the pretended quadrature of Simon à Quercu, he discovered the well-known and very correct proportion between the diameter and circumference, viz. 113 to 355; which has always been allowed to be very near the truth. About the same time, or a little after, Vieta, a French mathematician, in his *Universalium Inspectionum ad Canonum Mathematicum*, published in 1579, proved, by means of the inscribed and circumscribed polygons of 393216 sides, that if the radius be 1, the circumference will be very nearly expressed by 3.1415926535; and showed, that the latter number was too small, but if its last figure were increased by unity it would then be too great. Not long after, Adrian Romanus, a Flemish mathematician, carried this approximation to 16 places of figures; but this was far exceeded by Ludolph Van Ceulon, a native of the same country, who extended it to 35 figures, and proved at the same time, that if the last of these 35 figures were augmented by unity, the number would be too great. Of this labour, which was rather the exercise of his patience than of his ingenuity, he was so proud, that, after the example of the profound geometrician of Syracuse with respect to the sphere and cylinder, he requested it might be inscribed on his tombstone; and it is said, that this singular monument of human folly is still to be seen in one of the towns of Flanders. Willebrord Snell, the countryman of Metius, also made several additions to what had been previously done on the subject. He discovered and published, in his book styled *Cyclometricæ*, the method of expressing, by an approximate proportion, and a very simple calculation, the magnitude of any arch; and he made use of this method in examining the calculation of Van Ceulon, which he found to be correct. By this method he also calculated a series of both inscribed and circumscribed polygons, beginning with the decagon, and always doubling the number of sides, until the number was 5242880; and ranged the results of his computations in a table, for the purpose of detecting the falsity of any future pretended quadrature of the circle. The celebrated Huygens, when very young, enriched

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this measure of the circle with several new theorems; and successfully combated the pretended quadrature of Gregory Saint Vincent, a Jesuit of the Netherlands, who announced his discovery as only wanting a few calculations to render it complete, but which he dextrously forgot to perform. James Gregory and Leibnitz, about the same time, are said to have discovered, independent of each other, a very simple series for expressing the length of an arch of a circle, and which was first given in a letter of the 15th of February 1671, from Gregory to Mr. Collins. If a be an arc, t its tangent, and r the radius, then $a = t \times (1 - \frac{t^2}{3r^2} + \frac{t^4}{5r^4} + \frac{t^6}{7r^6} - \frac{t^8}{9r^8} +$ &c.). The arc must not be assumed greater than half a quadrant, otherwise the series will not converge. Dr. Halley also discovered a simple series for expressing the arc of 30°; which is $a = \sqrt{\frac{1}{3}} \times (1 - \frac{1}{3.3} + \frac{1}{5.3^2} - \frac{1}{7.3^3} + \frac{1}{9.3^4} -$ &c.); which converges very quickly, and being multiplied by 12, gives the whole circumference. Mr. Sharp, an English mathematician, in 1699, undertook the quadrature of the circle for his own private amusement, and deduced from two different series, by which the truth of it was proved, to 72 places of figures, as may be seen in the introduction to Sherwin's Tables of Logarithms. Mr. John Machin, professor of astronomy in Gresham college, discovered another very expeditious series for expressing the length of the circumference of a circle, depending on the differences of arcs, the tangents of which have certain relations to each other: but we believe it was by means of Dr. Halley's series that he extended Mr. Sharp's number to 100 places of figures. M. de Lagny, a French mathematician, continued this computation to 128 places of figures; respecting which Montucla has remarked, that "if we suppose a circle, the diameter of which is a thousand millions of times greater than the distance between the sun and the earth, the error in the circumference would be a thousand millions of times less than the thickness of a hair.". Nay, it is even possible to go farther than this, and Euler has pointed out the method of accomplishing it, in the Transactions of the Imperial Academy of Sciences at Petersburgh. We, however, hesitate not to say, that any such attempt ought to be considered as superfluous labour. Dr. Hutton has given several series for the same purpose, both in his Mensuration, and in his paper on that subject in the Philosophical Transactions for 1776, to which we must refer such of our readers as are curious about this subject.

While these and other mathematicians were extending the approximative methods for finding the circumference and area of a circle, others were endeavouring to obtain, and even asserting

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that they had obtained their *true* measures; nor were others wanting who doubted the possibility of ever attaining these. Of this latter opinion was the famous Dr. Barrow, who in his Mathematical Lectures observes, that the radius and circumference of a circle are lines of such a nature, as to be not only incommensurable in length and square, but also in cube, biquadrate, and all higher powers to infinity. His reasoning is, that the side of the inscribed square is incommensurable to the radius, and the square of the side of the inscribed octagon is incommensurable to the square of the radius; and consequently the square of the octagonal perimeter is incommensurable to the square of the radius: and thus the ambits of all regular polygons, inscribed in a circle, may have their superior powers incommensurate with the co-ordinate powers of the radius; whence the last polygon, that is, the circle itself, seems to have its periphery incommensurate with the radius. Newton too, in his Principia, has endeavoured to prove the impossibility of the general quadrature of oval figures by the description of a spiral, and the impossibility of determining, by a finite equation, the intersections of that oval and spiral, which must be the case, if the oval be quadrable. Other authors have also undertaken to demonstrate the impossibility of squaring the circle by any means whatever; and Legendre has proved, in the fourth note prefixed to his Geometry, that the ratio of the circumference to the diameter and its square, are irrational numbers.

We shall now conclude this historical sketch with some brief notices of the more amusing class, who have either persuaded themselves, or attempted to persuade others, that they had discovered the true quadrature of the circle, which so many abler mathematicians had so long sought in vain; and whose pretensions, like falling stars, attracted notice for a moment, and then, like them, sunk into eternal oblivion. The first, among the moderns, who pretended to have squared the circle, was Cardinal de Cusa. He rolled a cylinder over a plane, till the point which first was in contact with it touched it again; and then, by a train of reasoning, wholly destitute of geometrical precision, he endeavoured to determine the length of the line thus described. He was easily refuted by Regiomontanus, about 1465. Near a century after, Orontius Finæus attracted notice by his parallogisms on this subject, but the fallacy of his reasonings was clearly shown by Peter Nonius and J. Borelli. The celebrated Joseph Scaliger also ranks under this class. As he had no great esteem for geometricians, he endeavoured to show them his superiority, in undertaking, by way of amusement, the quadrature of the circle, and seriously imagined that he had obtained it: but Vieta, Clavius, and others, found no difficulty in refuting his reasonings. Longomontanus, the celebrated Danish astronomer, was also one of the number who asserted he had obtained a finite

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ratio between the diameter and circumference; which was exactly as 1 to 3.14185. Our countryman, Mr. Hobbes, also rendered himself remarkable as a member of this class; but his pretensions were refuted by Dr. Wallis. Olivier de Serres weighed a circle and a triangle equal to the equilateral inscribed triangle, and believed that the one was exactly double of the other. A very little knowledge of the subject, however, would have been sufficient to have shown him, that the double of this triangle is the hexagon inscribed in the same circle. A. M. Dethlef Cluver, in 1695, claimed the honour of this pretended discovery, and reduced the problem to one which is much simpler, and which he announced as follows: *Invenire mundum Menti divinæ analogum.* He also tried to prove, that Archimedes had been deceived in the quadrature of the parabola. About the middle of last century M. Liger increased the list of the deluded ones, by affirming, that he had solved this problem, by demonstrating, at least in his own terms, that the squre root of 24 was the same as that of 25, and of 50 equal to that of 49. Another pretender to discovery found out that curves ought not to be measured by comparing them with straight lines, but with curves; and thus supposed he had surmounted the difficulty: while M. Clerget discovered a property equally curious and interesting, viz. that a circle is a polygon of a determinate number of sides; and he thence deduced, what is perhaps still more curious, the magnitude of the *point* where two unequal spheres touch each other. M. Basselin, after performing calculations almost equally as laborious as those of Van Ceulen, imagined that he had obtained a ratio between the diameter and circumference of a circle beyond the limits assigned to it by Archimedes: M. B. however, though a professor in a university, did not know, till within a few days of his death, that the great geometrician of Syracuse had squared the parabola. The ridicule attached to these attempts and failures was chiefly confined to a small circle of the learned; but there were others who incurred it in a more extensive degree, and even paid the price of this extension. One Mathulon, who, from being a manufacturer of stuffs at Lyons, commenced geometer, claimed the merit of having solved this problem, and deposited 1000 crowns as a reward for the person who should prove that his solution was not correct. This was done by M. Nicole, a member of the French Academy of Sciences, who gave the reward to the general hospital at Lyons. A similar circumstance also happened some time afterwards. A Frenchman announced the quadrature of the circle, and challenged the whole world to refute him; and deposited 10,000 l vres for the person who should do this. This grand problem he reduced to the mechanical process of dividing a circle into quadrants, and then turning these with their angles outwards, so as to form a square, which he asserted to be equal to the circle.

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Three persons claimed the reward, and the cause was tried at the Chatelet of Paris; but the judges thought, that a person's fortune ought not to be diminished on account of the errors of his judgment, unless they were prejudicial to society; and the king decreed that the proposal should be void. The Academy of Sciences recommended him to study the elements of geometry; but he still fancied that future ages would blush for the injustice that was done him. M. le Rohberger de Vausenville, in a work, entitled "*Consultation sur la Quadrature du Circle*," inquires of mathematicians' if the quadrature of the circle would not be obtained if any means were devised for finding the centre of gravity of a sector of a circle in common parts of the radius and the circumference of the same circle. The meaning of this last clause is not clear; but it may be observed, that when M. de Vausenville can effect this without the arc being one of the terms, the business will be accomplished. The preceding examples appear to indicate what we may expect from future investigations of the subject; yet such is the weakness of many pretenders either to science or the immediate illumination of Heaven, that hopes are still entertained of attaining this quadrature, even within very narrow limits. Signor Rossi, an Italian attorney, visited London about five years ago, for the purpose of claiming the reward offered by this government for a correct method of ascertaining the longitude at sea, on the ground of his having squared the circle: he, however, seems to have fallen into the very same error as M. Liger; for he published a small work in Italian, in which he pretended to have demonstrated that the exact ratio of the side to the diagonal of a square is that of 5 to 7, which, in other words, is evidently saying that 50 is equal to 49. He also asserted, from either the same or a similar parallogism, that the diameter and circumference of a circle are to each other exactly as 5 to 16. Who would suppose this to be a result of the 19th century!

We shall now conclude this article, which we have already extended beyond our usual limits, with a few words respecting Mr. Wallace's paper, of which we have given as brief an abstract as possible. The investigations are conducted with a considerable degree of ingenuity, but the results are in general too complex to be either easily remembered, or readily applied to purposes of utility. The whole paper, which occupies 76 quarto pages, is extremely prolix, and might have been compressed into less than half the space, without losing any part of either its perspicuity or usefulness.

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On the Effects of westerly Winds in raising the Level of the British Channel. By JAMES RENNEL, *Esq. F.R.S.—Phil. Trans. Part II.* 1809.

MR. Rennel conceives, that "the fact of the high level of the Channel, during strong winds, between the W. and S. W. cannot be doubted; because the increased height of the tides in the southern ports, at such times, is obvious to every discerning eye: indeed, the form of the upper part of the Channel, in particular, is such as to receive and retain, for a time, the principal part of the water forced in; and as a part of this water is continually escaping by the Strait of Dover, it will produce a current, which must greatly disturb the reckonings of such ships as navigate the Strait, when thick weather prevents the land, or the lights of the Forelands, and the North Goodwin, from being seen.

"It is evident, that the direction of the current under consideration will be influenced by the form and position of the opposite shores at the entrance of the Strait; and as these are materially different, so must the direction of the stream be, within the influence of each side, respectively: for instance, on the English side, the current having taken the direction of the shore, between *Dungeness* and the *South Foreland*, will *set* generally to the north-east, through that side of the Strait. But, on the French side, circumstances must be very different; for the shore of *Boulogne*, tending almost due north, will give the current a like direction, since it cannot turn sharp round the point of Grisnez, to the north-eastward, but must preserve a great proportion of its northerly course, until it mix with the waters of the North Sea."

Another circumstance, which ought to be noticed, is, that the *shore of Boulogne*, presenting a direct obstacle to the waters impelled against it by westerly winds, occasions a higher level of the sea in its vicinity than in other places, and consequently a stronger current towards the Goodwin Sands. From this it may easily be inferred, that a ship passing the Strait in thick weather will be driven several miles northward of her reckoning. The regular tides, which are considerably affected by these winds, must also be taken into the account, in the application of these remarks.

Observations.—Of the general fact which Mr. Rennel has stated we conceive that there can be no doubt, as both the form of the Channel itself, and observation, confirm it; and the importance of having the direction, force, and comparative continuance, of these currents ascertained with all possible accuracy, merits the attention of all who have an opportunity of either making or communicating observations on the subject. We think it highly pro-

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bable, that many of the accidents which happen on the Goodwin Sands, during thick weather, arise from this cause; and the loss of the Britannia East India ship, which struck on the north-east extremity of the southernmost of these sands in January 1809, after it was concluded that she was quite clear of them, which Mr. R. has cited as an example, is one striking instance.

In a recent work, entitled "*Sailing Directions, &c. for the British Channel*," published by *Laurie* and *Whittle*, it is stated, that, in stormy weather, the extraordinary rise of the tide at the mouth of the Channel is ten feet. It is likewise stated, on good authority, that strong west winds cause the flood-tide to run an hour or more longer than at common times. The velocity of the spring-tides in this part is estimated at one mile and a half per hour, and that of the neap-tides at half a mile.

On the Attractions of homogeneous Ellipsoids. By JAMES IVORY, *A. M.—Phil. Trans. Part II.* 1809.

1. MR. Ivory's object in the present paper is to investigate expressions for the force with which a body, of a given figure and density, would attract a particle of matter, occupying any proposed situation; and he has confined his attention to homogeneous bodies, bounded by finite surfaces of the second order. Previous to entering upon these researches, he gives the following brief history of what has been done on the subject:

"The theory of the attractions of spherical bodies is delivered by Sir ISAAC NEWTON in the first book of the Principia. In the same place the illustrious author lays down a method for determining the attractions of round bodies (or such as are generated by the revolving of a curve about a right line which remains fixed), when the attracted point is situated in the common axis of the circular sections; and he employs this method to compute the attraction of a spheroid of revolution on a point placed in the axis. MACLAURIN was the first who determined the attractions of such a spheroid generally, for any point placed in the surface, or within the solid. The method of investigation, invented by that excellent geometer, is synthetical, but original, simple, and elegant, and has always been admired by mathematicians. When the attracted point is placed without the solid, the difficulty of solving the problem is greatly increased; and it was reserved for LE GENDRE to complete the theory of attractions of spheroids of revolution, by extending to all points, whether without or within the solid, what had before been investigated for the latter case only. LA PLACE took a more enlarged view of the problem; he extended his researches to all elliptical spheroids, or such solids whose principal sections are all ellipses; and he obtained conclusions with regard to them similar to what MACLAURIN and LE

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GENDRE had before demonstrated of spheroids of revolution. In this more general view of the problem the investigation is particularly difficult, when the attracted point is placed without the solid. The method of investigation which LA PLACE has employed for surmounting the difficulties of the last case, although it is entitled to every praise for its ingenuity, and the mathematical skill which it displays, is certainly neither so simple nor so direct as to leave no room for perfecting the theory of the attractions of ellipsoids in both these respects. LE GENDRE has given a direct demonstration of the theorem of LA PLACE, by integrating the fluxional expressions of the attractive forces; a work of no small difficulty, and which is not accomplished without complicated calculations. In the *Mécanique Céleste* the subject of attractions of ellipsoids is treated by *La Place* after the method first given by himself in the Memoirs of the Academy of Sciences for 1783, founded on the theory of series and partial fluxions."

2. Mr. Ivory then proceeds to the proposed investigations. The three co-ordinates, which determine the position of the attracted point, he denotes by a, b, c; the co-ordinates of the solid or attracting body, which are respectively parallel to the former, he expresses by x, y, z; and a molecule or element of this solid by d M. Then, by supposing the variable density equal to unity, and putting $f = \left\{ (a-x)^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{1}{2}}$ the distance of the molecule from the attracted point, the direct attraction of the molecule on this point will be $= \frac{d\,\mathrm{M}}{f^2}$. This force of attraction is next decomposed into other forces, acting in the direction of the three axes which are parallel to the co-ordinates, and directed to the planes from which the co-ordinates are computed, and the resulting expressions are,

$$\frac{d\,\mathrm{M}\,(a-x)}{f^3}, \text{ parallel to the axis of } x,$$

$$\frac{d\,\mathrm{M}\,(b-y)}{f^3}, \text{ parallel to the axis of } y,$$

$$\frac{d\,\mathrm{M}\,(c-z)}{f^3}, \text{ parallel to the axis of } z.$$

The amount of all the attractions parallel to the axis x is next denoted by A; and same thing for y and z by B and C respectively: then by restoring the value of f, and writing dx, dy, dz for its equivalent d M, there are obtained the three equations,

$$\mathrm{A} = \iiint \frac{dx \,.\, dy \,.\, dz \;\; (a-x)}{\left\{ (a-x)^2 + (b-y)^2 + (c-z)^2 \right\}^{\frac{3}{2}}},$$

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$$B = \iiint \frac{dx \,.\, dy \,.\, dx\,(b-y)}{\left\{(a-x)^2+(b-y)^2+(c-z)^2\right\}^{\frac{1}{2}}},$$

$$C = \iiint \frac{dx \,.\, dy \,.\, dx\,(c-z)}{\left\{(a-x)^2+(b-y)^2+(c-z)^2\right\}^{\frac{1}{2}}},$$

where the several triple fluents must be extended to all the molecules that compose the solid.

Each of these expressions is integrable with respect to one of the variable quantities it contains; and Mr. Ivory asserts that, when the integration is performed for the first of them, the result is

$$A = \iint dy \,.\, az \,. \left\{ \frac{1}{\left\{(a-x')^2+(b-y)^2+(c-z)^2\right\}^{\frac{1}{2}}} - \frac{1}{\left\{(a-x'')^2+(b-y)^2+(c-z)^2\right\}^{\frac{1}{2}}} \right\};$$

in which x' denotes the greatest value of x on the positive side of the plane, and x'' that on the negative side. The fluxion under the sine of double integration denotes the attraction which a prism of the matter of the solid, the length of which is $x' + x''$ and its base $dy + dz$, exerts on the attracted point, in the direction of the length of the prism. For the sake of brevity, the two denominators in the above fraction are put equal to Δ and Δ' respectively, and the expression then becomes $A = \iint dy \,.\, dz \,. \left\{\frac{1}{\Delta} - \frac{1}{\Delta'}\right\}$: this double fluent is to be extended to all the points, or indefinitely small spaces $dy \,.\, dz$, that compose the principal section of the solid made by the plane of y and z.

The values of B and C are to be integrated, and the fluents obtained, extended in the same manner.

3. The general equation of a surface of the second order bounding a finite solid, is

$$\frac{x^2}{k^2} + \frac{y^2}{k'^2} + \frac{z^2}{k''^2} = 1:$$

when the three denominators are all equal, the solid is a sphere; when two of them are equal, it is a solid of revolution; and when they are all unequal, it is an ellipsoid, or spheroid, having all its three principal sections ellipses. The result of the investigation contained in this section, Mr. I enunciates in the following theorem:

"If two ellipsoids of the same homogeneous matter have the same eccentricities, and their principal sections in the same planes;

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the attractions which one of the ellipsoids exerts upon a point in the surface of the other, perpendicularly to the planes of the principal sections, will be to the attractions which the second ellipsoid exerts upon the corresponding point in the surface of the first, perpendicularly to the same planes, in the direct proportion of the surfaces, or areas, of the principal sections, to which the attractions are perpendicular."

4. Mr. Ivory next considers the concluding formula in the second section for the attractive force parallel to the axis k, on the supposition that the point is within the ellipsoid. His resulting expressions, in this case, are

$$A = a + \iint \frac{2x \,.\, dy \,.\, dz}{\left\{x^2 + y^2 + z^2\right\}^{\frac{3}{2}}}$$

$$B = b + \iint \frac{2y \,.\, dx \,.\, dz}{\left\{x^2 + y^2 + z^2\right\}^{\frac{3}{2}}}$$

$$C = c + \iint \frac{2z \,.\, dx \,.\, dy}{\left\{x^2 + y^2 + z^2\right\}^{\frac{3}{2}}}$$

The several fluents in these expressions are to be extended to the whole of the surfaces of the principal sections, to which the attractions are perpendicular.

Similar, but rather more complex expressions are then obtained for the case where the attracted point is without the ellipsoid.

5. The result of the investigation in this section, the author states in the following theorem:

"If a point be situated within a shell of homogeneous matter, bounded by two finite surfaces of the second order, which are similar and similarly placed; then the attraction of the matter of the shell upon the point in any one direction, will be equal to, and destroy, the attraction of the same matter in the opposite direction."

6. Mr. Ivory conceives that nothing more is now wanted to complete the theory of the attractions of homogeneous ellipsoids, than to integrate the fluxional expressions of the last set of equations. After an investigation of this nature for an ellipsoid in general, he observes: "Thus the two cases, when the attracted point is within the solid or in the surface, and when it is without the solid, differ only in the limits of the fluents: in the former case, the fluents beginning when the variable quantity is infinitely great, are to be extended till it has decreased, so as to be equal to the least of the semi-axes of the given ellipsoid; and, in the latter case, the fluents are to be extended only till the variable quantity has decreased, so as to be equal to the least of the semi-axes of the ellipsoid, whose surface passes through the attracted point. In the former case, the values of the fluents are the same

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for all points within the ellipsoid, and in its surface; in the latter case, these values depend upon the attracted point."

Making $k'^2 = k^2 + e$, and $k''^2 = k^2 + e'^2$, the following formulas express the attractions of an oblate spheroid upon a point within the surface, acting parallel to a, b, c, the co-ordinates of that point, of which a is parallel to the axis of revolution.

$$A = \frac{3\,a\,M}{e^3} \cdot \left\{ \frac{e}{k} - \text{arc. tan.} \frac{e}{k} \right\}$$

$$B = \frac{3\,b\,M}{2\,e^3} \cdot \left\{ \text{arc. tan.} \frac{e}{k} - \frac{\frac{e}{k}}{1+\frac{e^2}{k^2}} \right\}$$

$$C = \frac{3\,c\,M}{2\,e^3} \cdot \left\{ \text{arc. tan.} \frac{e}{k} - \frac{\frac{e}{k}}{1+\frac{e^2}{k}} \right\}.$$

M denotes the mass of the ellipsoid.

The formulas for an oblong spheroid, under similar circumstances, are

$$A = \frac{3\,a\,M}{e^3} \cdot \left\{ \tfrac{1}{2} \cdot \text{hyp. log.} \left(\frac{k+e}{k-e}\right) - \frac{e}{k} \right\}$$

$$B = \frac{3\,b\,M}{e^3} \cdot \left\{ \frac{\frac{e}{k}}{1-\frac{e^2}{k^2}} - \tfrac{1}{2} \cdot \text{hyp. log.} \left(\frac{k+e}{k-e}\right) \right\}.$$

$$C = \frac{3\,c\,M}{2\,e^3} \cdot \left\{ \frac{\frac{e}{k}}{1-\frac{e^2}{k^2}} - \tfrac{1}{2} \cdot \text{hyp. log.} \left(\frac{k+e}{k-e}\right) \right\}.$$

Directions are also given for obtaining formulas for these attractions when the point is situated out of the ellipsoid.

Observations.—Mr. Ivory informs us, that it was in the study of *La Place*'s Mécanique Céleste that the method which he has followed in this paper was suggested; and we think that its internal evidence is sufficiently strong to support the assertion, and even to remove all doubt from the mind of the sceptical reader. We do not recollect to have seen any paper, published in this country, which bears more evident marks of the French school; and we suppose it was there also where Mr. I. learned that this difficult branch of physical astronomy which he professes to sim-

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plify stood in no need of assistance from the use of diagrams. While we readily admit that this product of Mr. Ivory's mathematical skill exhibits his extensive acquaintance with the modern analysis, proves the fertility of his genius in expedients for managing its abstruse parts, and shows his knowledge of the subject which he has chosen for the exercise of his ingenuity, we must be allowed to express our opinion that he has rather sketched the method of these investigations than performed them; and that very few of the readers, into whose hands this paper may fall, will be able to follow him through its contents; and some of these will be deterred from this undertaking by the time and labour necessary to fill up the chasms it contains, in order that they may pass on with that satisfaction which mathematical evidence is so well calculated to afford. Perhaps this was Mr. I.'s object; if so, he has ingeniously succeeded.

Description of a reflective Goniometer. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S.—Phil. Trans. Part II.* 1809.

DR. Wollaston remarks, that from the late advances that have been made in crystallography, a great many mineral substances may now be known, if we can only ascertain their angular dimensions; but when the crystal is small, and the radius in contact with its surface necessarily very short, our goniometers do not possess sufficient accuracy to determine these with the requisite precision. A means of remedying this defect occurred to Dr. W.; and for this purpose, the ray of reflected light from the surface is employed as radius, instead of the surface itself; and accordingly for a radius of $\frac{1}{50}$ of an inch, we may substitute either the distance of the eye from the crystal, which would naturally be about twelve or fifteen inches; or, for greater accuracy, we may, by a second mode, substitute the distance of objects seen at a hundred or more yards from us. The instrument which he uses consists of a circle graduated on its edge, and mounted on a horizontal axle, supported by an upright pillar. This axle being perforated, admits the passage of a smaller axle through it, to which any crystal of a moderate size may be attached by a piece of wax, with its edge, or intersection of the surfaces, horizontal and parallel to the axis of motion. This position of the crystal is first adjusted, so that, by turning the small axle, each of the two surfaces, whose inclination is to be measured, will reflect the same light to the eye. The circle is then set to *zero*, or 180°, by an index attached to the pillar that supports it. The small axle is then turned till the further surface reflects the light of a candle, or other definite object, to the eye; and, lastly (the eye being kept steady in the same place), the circle is turned by its larger axle, till the second surface reflects the same light. This second sur-

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face is then ascertained to be in the same position as the former surface had been. The angle through which the circle has moved is in fact the supplement to the inclination of the surfaces; but as the graduations on its margin are numbered accordingly, in an inverted order, the angle is correctly shown by the index, without need of any computation."

In this method, when the eye and candle are only about ten or twelve inches distant, a small error may arise from parallax; but when the surfaces are sufficiently smooth to reflect distinct images of objects, this error may be entirely obviated by another method. For this purpose, if the eye be brought within about an inch of the reflecting surface, the reflected image of some distant chimney may be seen inverted beneath its true place, and by turning the small axle may be brought to correspond apparently with the bottom of the house (or with some other distant horizontal line). In this position the surface accurately bisects the angle, which the height of that house subtends at the eye (or rather at the reflecting surface); then, by turning the whole circle and crystal together, the other surface, however small, may be brought exactly into the same position; and the angle of the surfaces may thus be measured with a degree of precision which has not hitherto been expected in goniometry.

Observations.—The name of Dr. Wollaston is familiar to many of our readers from its frequent occurrence in our pages; and we are inclined to think, that the object of this communication will not be found inferior, in either accuracy or importance, to several of those which have been the results of former exertions of his ingenuity. He conceives the accuracy of this instrument to be such, that several errors in former observations may be corrected by it; and he instances one in the common carbonate of lime. The inclination of the surfaces of a primitive crystal of this kind is stated at 104° 28′ 40″; but which he determines to be very nearly, if not accurately, 105°.

An Account of a very extraordinary Effect of Refraction, observed at Ramsgate By the Rev. S. Vince, *A. M. F. R. S.—Edin. Trans. Vol. VI. Part II.*

In this account Mr. Professor Vince states, that the tops of the four turrets of Dover castle are usually seen above the top of the hill lying between Ramsgate and Dover; but on the 6th of August 1806, about seven in the evening, when the air was very still and a little hazy, not only the turrets, but also the whole of the castle, appeared to be situated on that side of the hill next to Ramsgate, and rising as much above it as usual. This phenomenon was so very singular and unexpected, that Mr. Vince at

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first thought it was some illusion; but, on continuing his observations, he was satisfied it was the castle. He then gave the telescope to a person present, who examined the appearance attentively, and with the same result. These observations were continued for about twenty minutes, during which time the appearance remained precisely the same. At this time rain coming on, put a stop to any further observations. Between the observers and the land from which the hill rises there was about six miles of sea, and near the same distance from thence to the top of the hill: they were also about seventy feet above the surface of the water. The hill itself did not appear through the image, as might have been expected. "Our attention," Mr. Vince adds, "was of course principally directed to the image of the castle; but if the hill behind had been at all visible, it could not have escaped our observation, as we continued to look at it for a considerable time with a good telescope." Since, however, the parts of the hill on each side of the castle seem to have appeared in their usual situation, the Professor is of opinion, that the rays of light from the hill immediately before the castle must also have retained their ordinary direction.

"A phenomenon of this kind," he says, "I do not remember to have seen described; and it must have been a very extraordinary state of the air to have produced it. It is manifest, that a ray of light, coming from the top of the hill, must have come to the eye in a curve lying between the two curves described by the rays coming from the top and bottom of the castle, in order to produce the effect."

Mr. Vince illustrates this position by a diagram, showing the relative situation of the three curves; and accounts for this singular appearance by a quick variation of atmospheric density.

Observations.—The circumstance of no phenomena of this kind having been recorded, we think, with the Professor, is a proof that the constitution of the air at the time of observation must have been such as rarely happens; otherwise similar appearances would have been previously noticed. But we must confess, that we are by no means perfectly satisfied with the explanation which Mr. Vince has attempted to give of the path of the rays of light. We can conceive it barely possible, that "a ray of light coming from the top of the hill" might "have come to the eye in a curve lying," at its entrance into the eye, "between the two curves described by the rays coming from the top and bottom of the castle:" but we conceive it to be absolutely impossible, that, under such circumstances, the *middle* parts of the castle should have retained their relative situation, so as to *coincide* in their apparent place with the top of the hill; for, in order that the rays from these two objects should enter the eye in the same direction, they must have proceeded in the same direction throughout their pas-

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sage, which is impossible; nor do we think it possible that any effect of atmospherical refraction can ever bring an image of one object exactly upon the actual apparent place of another, as Professor Vince seems to suppose must have happened, without having actually observed it. If such a circumstance should again be observed, it would be desirable that the angular positions should be ascertained by a quadrant. It is not improbable, that the lower part of the apparent figure of the castle was an inverted image, and that the ground seen on each side of it was not a part of the interposed hill, but some object much more remote; we will not, however, pretend to insist on this conjecture, any further than as a hint which may deserve consideration.

REVIEW OF SPECIFICATIONS OF PATENTS,

PUBLISHED IN THE REPERTORY OF ARTS, MANUFACTURES, &c.

Mr. WILLIAM FRANCIS SNOWDEN's *Patent for certain Improvements in an Engine for cutting Hay and Straw into Chaff, and for cutting other Articles. Dated February* 1808.—*Repertory of Arts, No.* 89, *Second Series.*

MR. Snowden states his improvement to consist in two separate parts, viz. a new, more simple, and better way of bringing the straw to the knife; and a new mode of compressing the article while the knife is cutting it. In order to accomplish the first of these objects, instead of the straw being made to slide forward on the bottom of the box which contains it, as in the old machines, in this the box brings the straw, or other article, forward with it to be cut; and while the cutting is performed, a retrograde motion is given to the box by a projecting part of a lever acting against a regulating nut. By this means the cut is regulated to any length, and the box is caused to move forwards by the same lever, as the knife rises, by acting against two pieces fastened for that purpose to the inside of the box.

For the purpose of effecting the better compression, the lever, already mentioned, is made to act on a pin in the frame of the compresser. To the inner end of this lever is fixed a weight and a roller; at the other end is the connecting-rod, by which the motion of the knife is communicated to the lever, and also the weight of the lever; all of which act as a weight or pressure upon the article while the knife is cutting it. When the knife rises, the roller and weight at the inner end of the lever act as a counterpoise to the opposite powers, till the roller meet the article in the box, when the lever changes its fulcrum to the roller, which suffers the compressor to rise, and the article to be brought forward to be cut. The machine may be worked either by a lever or by a fly-wheel.

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Observations.—We apprehend that these improvements will not be found to possess so much value as the patentee appears to think they do. We do not conceive the advantage which is to arise from the double motion of the box, or in what way the sliding of the box back while the knife is in the act of cutting the straw, is preferable to the method of bringing the straw forward for each cut, while the box remains fixed.

Messrs. RICHARD TREVETHICK *and* ROBERT DICKINSON'S *Patent for a new Method or Way of stowing Cargoes of Ships and other Vessels by means of Packages for containing Goods and Products of certain Descriptions destined for Conveyance by Sea, not hitherto employed. Dated October* 1808.—*Repertory of Arts, No.* 91, *Second Series.*

INSTEAD of the wooden packages in common use at present for containing several kinds of merchandise, these patentees make them of iron either by casting into the form required, or by rivetting together plates of the same metal; to these iron vessels or receptacles covers are adapted, capable of being secured by screws, bolts, or other known means, so as to render the vessels secure, and impenetrable to the external air or moisture. These packages are made of such forms as to fit each other exteriorly, and without that waste of space which takes place in the stowage of wooden casks. In order to accomplish this point in as complete a manner as possible, the rectangular or hexagonal prism forms are preferred to all others. But as the better preservation of the contents of such packages is another advantage that will result from their use, for some purposes they are tinned on the inside, or coated with a varnish suited to the commodities they are destined to contain.

Observations.—It is sufficiently evident that considerable space might be saved, and consequently more comfort afforded those on board, by the use of this kind of packages, than by those made of wood. And we think with the patentees, that their adoption would be attended "with advantage to the proper trim of carrying sail, and the safe and expeditious performance of their respective voyages." We apprehend, however, that they will meet with several obstacles to their introduction to general use.

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Mr. MARK DOBITO's *Patent for a new and improved Plough for underdraining Land. Dated June* 1809.—*Repertory of Arts, No.* 90, *Second Series.*

This *invention* of Mr. Dobito consists of a plough and machine by which it is drawn forward. This comprises a beam, handle, cutting counter (coulter we suppose), and mole, and two rollers or wheels to keep the drain at an equal distance from the surface of the ground. The machine employed for drawing the plough forward, consists of a frame bearing a vertical axle of a conical shape; it is provided with anchors and props for fixing it firmly to the ground, and with wheels for moving it forwards. The axle is turned by a lever moved by the force of either horse or men; and a rope or chain attached to the beam of the plough, and winding round the axle by its motion, causes it to advance and form the drain.

Mr. WILLIAM FRANCIS HAMILTON's *Patent for preparing Soda and other mineral Waters, spirituous, acetous, saccharine, and aromatic Liqueurs. Dated May* 1809.—*Repertory of Arts, No.* 91, *Second Series.*

VARIOUS solutions of alkalies, salts, &c. and combinations of ardent spirits, vegetable acids, aromatic oils, sugar, milk, mucilage, are used for saturation and supersaturation with carbonic acid, which is obtained from whiting, by dilute sulphuric acid. The whiting is used and the gypsum is let off in a liquid state.

The liquor to be impregnated is placed in a strong well-tinned copper vessel, and agitated in a cold bath, suspended by pendulous tubes. The impregnation goes on continually, fresh liquor being supplied by a condensing syringe, and the impregnated liquor drawn off by a decanting stop-cock into a long ovate bottom, which must always be laid on its side, so that the liquor keeps the stopper air-tight.

The pressure of 120 square inch is generally used, which is somewhat reduced on the liquors being bottled.

The copper vessel used for impregnation has a double set of stop-cocks, that the process may not be interrupted, while one set is repaired.

The decanting is performed by passing the decanting tube through the perforated stopper of the bottle, by which the aerated liquor passes until its entrance is resisted by the pressure of the contained air and gas extricated in the decanting, which is then suffered to escape, and the decanting continued as before. When

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the bottle is full, it may be withdrawn and stopped by letting off slowly, a small portion of the gas contained before its removal. But sometimes the decanting tube is so constructed as to allow the stopper to pass into the bottle previous to its removal, from the flat air-tight fitting.

Mr. JOHN DAVENPORT'S *Patent for ornamenting all Kinds of Glass in Imitation of Engraving, or Etching. Dated July* 1806. —*Repertory of Arts, No. 92, Second Series.*

THE usual method of ornamenting glass in this manner has been to roughen the surface by grinding, and then to polish different parts in borders, figures, &c. and blind windows have hitherto been made by grinding off the polished surface of the panes. The present patent proposes to effect these objects by laying an additional coat on the glass.

The compositions fittest for this purpose, are, 1. 160 parts of cullitt or broken glass, 10 of pearl-ashes, 40 of red lead or litharge, and 10 of arsenic. 2. 120 parts of cullitt, 160 of red lead or litharge, 60 of sand, and 60 of borax. 3. 70 parts of red lead or litharge, 22½ of sand, and 40 of calcined borax. Or any other mixtures of the like quality. These compositions being fused, are to be ground to a very fine powder; which is to be mixed when used with a solution of one part of double-refined sugar in two of water, and about one third of common writing ink, or of a watery solution of calcined copperas: the last ingredients act in the same manner as oxide of manganese in preventing the cloudiness or milkiness of the glass.

The powdered composition is thus to be laid on the glass evenly, and when dry, the requisite designs are to be worked out by some proper tool, observing to breathe upon the glass if the composition should be got too dry. When the design is finished, the glass is exposed to a proper degree of heat, so as to produce a semivitrification of the coating, and incorporate it with the surface of the glass, and thus a roughness in imitation of that produced by grinding will be obtained.

This method is peculiarly adapted to produce beautiful specimens of art for the windows of altar-pieces, libraries, museums, and ornamental buildings of all descriptions.

When essential oils, gums, resins, and many other substances were used to lay on the coating, it became so fastened on the surface of the glass that the designs could not be etched out with facility and precision.

Oxide of manganese, or perhaps some other oxides, might be applied instead of common ink, or copperas; but these articles are cheap and sufficiently efficacious.

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Observations.—We cannot but consider this as a very excellent process for the purpose intended, and far superior to the process of roughening the glass by grinding, and then polishing designs upon it, both in speed, and delicacy of execution.

Messrs. PHILLIPS LONDON, *Sen. and Jun. Patent for new and improved Methods of manufacturing, refining, and purifying Muriate of Soda, or common Salt. Dated April* 1809.—*Repertory of Arts, No.* 90, *Second Series.*

THIS method of manufacturing common salt consists first in calcining, or even melting the salt in a reverberatory furnace, or any proper pots, and secondly, in adding to the salt at any period of its refining the necessary materials to effect the separation of the earths, metals, and earthy or metallic salts mixed with it. The materials particularly enumerated by the Messrs. London, are the three alkalies, and lime, with their nitrates, sulphates, or carbonates, or any other of their combinations. These substances might also be mixed with a solution of the salt.

If the common salt is purified by fusion, either by itself, or with any appropriate addition, it may either be poured off the impurities while liquid, or left to cool undisturbed. In either case the purified salt may be broken down into a convenient size for use, and the salt that remains among the impurities may be separated by elixiviation.

The patentees think that the common salt which has been purified in this manner may supersede the necessity of using any foreign bay salt.

Observations.—The calcination, or, as it is technically called, the decrepitation of salt, has been practised for ages; so that there remains only the purification by means of fusion which can be said to be peculiar to Messrs. London.

It is probable, however, that decrepitated salt, or ground melted salt, especially if previously purified when in solution, might supersede the use of foreign bay salt.

Mr. THOMAS COBB's *Patent for certain Improvements in the Art of making Paper in separate Sheets. Dated December* 1807.—*Repertory of Arts, No.* 90, *Second Series.*

IN his method of making paper, Mr. Cobb uses a large vat in which the pulp is prepared, and from which it is suffered to flow occasionally into a smaller one for the immediate purpose of manufacture. In both these vats agitators or hogs are placed to keep the pulp duly suspended. The pulp is permitted to flow out of

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the smaller vat by a regulated aperture, and over an inclined plane into the paper-moulds, which are supported upon an endless web, or, in preference, two or more pieces of girting, or any other agitable material. This web, or girting, is stretched over two rollers at the ends of a horizontal frame; and the revolution of these rollers conveys away the moulds in regular succession as they receive the pulp. "And the said horizontal frame is during the working of the machinery kept in a constant state of rapid horizontal agitation by means of a crank or snail wheel and re-acting spring, or by other fit means."

The method of using this machinery is thus described. A workman places the moulds in succession upon that end of the web or girting which is beneath the smaller vat; these pass on to receive the flow of pulp, and are kept in a state of constant agitation till they arrive at the other end of the web, where they are taken off by another workman, and couched in the usual or in any other manner. The moulds are then returned by means of another circulating web to the first workman.

This patentee also describes machinery for facilitating the operations of couching and pressing. For this purpose a table faced with felt is placed beneath a circulating web, moving in the manner above-mentioned, to support the pressure of the mould during the act of couching. The web then passes between a pair of cylinders, the upper one of which is covered with felt, which press the sheet in its way to the workman who takes it off, and lays it in the usual way. Mr. C. then describes other machinery by which the business may be performed without the assistance of a coucher. This is effected by causing the mould with its charge to pass under the roller of a descending web, which being thus applied to the sheet of paper, takes it up; and from which, after having been conducted between one pair of cylinders, or more if necessary, it is taken off as before.

The deckle or rim of the mould is likewise differently constructed from those at present in common use. The wooden frame is made equally thick at the bottom, but brought to an edge at the top, to suffer the water to run off. Instead of nailing the wire on the top of the mould, it is turned down the side, and fastened by covering the whole outside of the frame with thin metal. Another square frame of wood or metal is then put over this, "which slides up and down, and forms the deckle, which by pressure or re-action from the bearing parts of the agitable frame is made to stand a little above the face when the said mould is placed in a situation to receive the charge, and is suffered to slide back for want of the pressure or re-action when the mould arrives at the web of the couching machinery." Moulds of this kind are said to apply much closer to each other upon the frame, and thus to prevent a considerable waste of stuff which would otherwise take place.

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Observations.—The machinery of Mr. Cobb appears to us to have little claim to originality of invention, the principle being precisely the same as that of Mr. Fourdrinier (see our third volume, p. 207), and the alterations in the modification are not such as to render it more simple of construction, or more efficacious in use; and, independent of these fundamental objections to the machinery, we think there are many important defects in Mr. Cobb's specification. First, he gives no datum whatever as to the rate that the moulds are carried. Secondly, he omits to state in what direction, or with what degree of rapidity, or with what extent of oscillation, the frame by which they are carried is to be shaken. Thirdly, he does not explain whether there is any mode of attaching his moulds to the endless web which carries them under the pulp, and this appears to us to be necessary to cause the movement to be communicated to the mould, as otherwise the quickness of the shake (for the vibrations ought to be about 150 per minute to make paper well) would prevent the mould being affected by it; the weight of the mould lying upon the endless felt or straps would not constitute a sufficient purchase, without some mechanical fixation, to ensure a regular vibratory motion being given to it. Fourthly, the moveable frame, the apparatus for giving the shake, the mode of constructing the deckles of the mould, and the formation of the "web, or girting, or straps, or chains, or rollers," on which the moulds are carried, is not described, but left to be found out by those who make the machine, and yet surely there can be nothing more important than the means of conducting the moulds and keeping them perfectly level. In the machinery for pressing and couching, Mr. Cobb has not described the apparatus, by means of which a workman might with his foot throw a pair of pressing cylinders in and out of gear as rapidly as the sheets of paper would succeed each other to the hands of the coucher. Such an operation appears to us impracticable.

We have further to remark, that the illustrative drawing is not at all calculated to compensate for the defects and omissions in the written part of the specification.

The perusal of this specification induces us to advert to the opinion entertained and acted upon by many patentees, namely, that it is only necessary to describe the principle of an invention, without minutely explaining the construction of the machine founded upon it. As it is the plan and object of this work to afford information on every point connected with patents for inventions, we deem it consistent with that view to introduce a few remarks, tending to show the fallacy and danger of such an opinion.

The law, permitting to the Crown the privilege of granting patents of monopoly for new inventions, is intended for the public benefit; the reward it offers is held out as an incitement to call genius into exertion for the advantage of the community. Every

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patentee ought to bear in mind, that the monopoly granted him is the price paid by the public for his discovery, and the patent is made on condition of the public being put in complete possession of it, that is to say, the specification required by the letters patent must be made in such manner, that a competent workman may be enabled to construct a machine capable of performing what the title sets forth, without any invention of his own, and without requiring any further instruction than what the specification affords. As the validity of the patent depends on the correctness of the specification, it behoves the patentee to bestow all his care and ability on this object.

To draw up a specification, which shall contain a luminous and minute description of the machine, without limiting the patentee's privilege to any particular modification of the principles on which it is founded, is a task which requires not only talents and technical knowledge, but great experience in the nature of patent-right; and those who have such an undertaking before them, will do well to avail themselves of any assistance they can procure to contribute to its being ably done; for, provided an invention be original, the patentee's security can be affected by nothing but an injudicious or imperfect specification.

The business of paper-making, which is now a very extensive and important branch of our manufactures, has recently much attracted the attention of ingenious men, and a great deal has already been done towards the substitution of machinery for manual labour. We understand, that within the last eight years it has superseded the ancient method to the extent of about a twentieth part of the quantity made in this island; and as wages are most likely to advance, and machinery may be expected (owing to the knowledge of it that is derived from general use and experience) to be improved and simplified, we have little doubt that a very few years will effect a revolution in this manufacture, similar to that which took place in the cotton trade in consequence of Arkwright's inventions. The endless process appears to us to be most simple, and likely to be adopted to considerable extent in a short period; but, although the process of using a series of moulds is a delicate and complicated operation, we have little doubt that it will be brought to a high degree of perfection, and if so, it will afford the opportunity of manufacturing every description of paper by machinery. In how short a time this may be effected we cannot presume to predict, but we anticipate the period with pleasure, as it would be a source of public benefit, and a great triumph of human ingenuity.

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AGRICULTURE.

Facts and Observations relative to the Variety of Irish Grass called Fiorin; in a Letter from WILLIAM RICHARDSON, *D. D. late Fellow of Trinity College, Dublin, to Thomas Allen, Esq.— Farmer's Mag. No.* 40.

THIS paper may be considered *supplementary* to an article noticed in the Supplement to the last volume of the RETROSPECT from the Communications to the Board of Agriculture.

After stating the history of the fiorin grass in a more succinct way than in that communication, Dr. Richardson states, that being desirous in 1807 to bring part of an unproductive field into cultivation, he planted potatoes in it with the usual quantity of dung, and when they were dug out late in September, laid it down with fiorin. Late in the same month of 1808 the stripe of ground thus laid down was mowed, and it was the finest meadow ever cut, and the crop seemed double the quantity usually produced on so much ground; the following year the ground was mown again, and the crop found much superior; for though the strings were not one third of the length of those of the former year, yet this was more than compensated by the prodigious increase of their number. Another stripe was laid down with this grass at right angles to the former, which proved equally luxuriant.

The proper times for laying down fiorin grass are stated to be October, November, and December; for though it will thrive well, if laid down with roots or strings any day in the year, if the grass itself be only looked to, yet where crops are the object, other circumstances must be taken into consideration; and then the periods named will be found best calculated for its success, because in those months it has fewer weeds to encounter, which might otherwise prove an impediment to its growth. It will, however, come on well in January, February, and March; and even put down in April and May, will succeed on rich ground, if kept free from weeds, and give a tolerable crop that year.

Fiorin seed is represented as less fit for the purposes of propagation than pieces of the root, because the young tendril of the seedling plant is so small, and so slow of growth, that it is almost impossible to protect it from weeds and other grasses, which do not mark their species early; and in general practice seed is considered inadmissible.

With respect to *soils fit for fiorin culture*, it is asserted that the fiorin thrives equally well on all soils, the wettest and the driest; for though the word equally may not be strictly proper if applied

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to luxuriance, yet it is if applied to the health of the plant: and instances are given of its thriving in wet marshy meadows, and on the top of park and garden walls, and on rocks almost bare, though luxuriance is not to be expected in such situations, though no doubt is entertained of its becoming so in meadows, and effectually protected; and it is found to increase its roots most abundantly in September, though the periods of vegetation are found very different; they vegetate rather more in October than in August, and increase in November, and continue growing in December.

The season for mowing fiorin and making it into hay is next treated on, and winter is declared to be preferable to summer for this purpose. Dr. Richardson says, "I would mow and make hay on the first and fifteenth of every month, from October to March inclusive." It does not, however, appear, that he would mow the same spot twice during that period.

The *quantity* of hay is stated to be on one English acre, unmanured, six tons, and on an acre which had been manured seven tons four hundred weight and upwards; and the *quality* far superior to common hay.

As *winter green food*, the value of fiorin is stated to have been established by practice. After the crop has been mown, a considerable quantity remains for milch cows, which both enriches their milk and increases its quantity; and when cows can obtain the grass, they persist in refusing dry hay.

The process of making hay at Christmas is thus described: The hay is opened in the first stirring wind, which soon carries off its exterior damp, whether acquired from rain or lying on the ground; the internal juices are anxiously retained. As soon as the hay is dry it is put into small cocks, when it is considered safe from all danger. In the following week the cocks are opened for half an hour, to let the air pass through them; and then eight or ten of these are put into a large cock, which will stand the winter safely; but it is considered more prudent to open these again in another windy day, and put them into cocks as large as men can build without treading on them; and these, secured by ropes or strings across the top, may remain till wanted for use, and need not be removed, as their surfaces will not turn white, nor their bottoms rot, as happens to other haycocks; and by these means the expense of bringing home and housing the hay will be saved: and these advantages are thought fully to compensate for the loss of the aftermath, which after winter mowings is necessarily given up.

Observations.—The facts contained in this article, as well as the memoirs of the fiorin grass noticed in the preceding volume, page 472, are well worth the attention of British agriculturists on both sides the Tweed, as this grass must be the best possible herbage for hilly and mountainous districts, if it affords only a

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greater part of the advantages held out by its patron, Dr. Richardson. It would, however, be very satisfactory to have its virtues ascertained by experiments both in England and Scotland, as a knowledge of its success here as well as in Ireland would have a more powerful tendency to its general introduction, than all that can be written on the subject by Dr. Richardson: it is only justice to him to remark, that he professes his readiness to answer all inquiries on the subject, addressed to him at his residence at Foy, in Ireland.

On planting Scotch Firs, and other Timber Trees.—By Mr. J. Borthwick.—*Farmer's Mag. No.* 40.

After quoting the greater part of a paper on this subject, published some years ago by James Farquharson, Esq. of Invercauld, Mr. Borthwick gives it also as his opinion, that the finest forests of firs are in glens, or on the sides of hills generally lying to a northerly aspect. He differs, however, from the writer quoted as to the cause of this more luxuriant growth in such situations, for Mr. Farquharson attributes it to their having a much shorter time of vegetation than a more southern exposure, or the open low countries; but our author conceives the cause to be, that in these situations the trees are not exposed to the south-easterly and westerly winds, which are the strongest winds in Scotland: for he considers, that no trees thrive well in an exposed situation, and that the fir is of all trees the most liable to injury from wind, on account of the brittleness of the wood, and the slender hold which the root takes in the ground; and besides these, the top is always bushy when the trees stand thick in a plantation, and consequently a greater surface is exposed to the wind at that extremity where it has the most power. The idea that firs become timber in a shorter space of time than other wood is successfully combated, as far as arguments can be received without the test of experience; and it is inferred, from observations made in almost all situations, that whenever firs succeed well, other trees would succeed equally well; and it appears to him, that the only advantage of planting firs is for the purpose of protecting and training up young hard-wood trees, which he thinks ought always to be interspersed among them; and as these grow up, the firs to be gradually pruned and taken out, till they are almost all cut down, to give room to the hard wood, which will not otherwise thrive; and when this is not the object, he considers the planting of Scotch fir a prejudicial practice.

Some directions are then given for preparing the ground for planting firs, and on the mode of planting and rearing them; and instead of having the ground in a pulverized state, as is the common practice, this writer thinks it better to have the ground in

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ley, and to plant the trees by making pits, which is less trouble, and not so apt to promote the growth of weeds, and the firm ground holding the plants better when the roots penetrate beyond the pits. It is recommended to let the firs stand thick in the seed-beds for two years, and then transplant them for a year, after which they may be removed to their final destination, where care should be taken not to plant the roots too deep; and planting in rows is preferred to planting them irregularly, as in the former way they may be the more easily thinned by taking out every second or third row; and the hard wood should also be planted in rows, with intervals of three or four rows of firs, so that ultimately the hard wood may remain in its proper and most advantageous situation.

Observations.—Though this paper does not contain any new information on the subject of planting firs, except the advantage derived from sheltering the young plants from the south and west winds; yet the practical remarks are very judicious, and are deserving the attention of the inexperienced planter.

On Potatoes. By the Rev. Mr. CAMPBELL.—*Farmer's Magazine, No.* 40.

THIS gentleman notices a paper from a correspondent in an early number of the Magazine, asserting, that plucking the blossoms off potatoe stems would double the produce of the crop, and states that he has tried the experiment *four* times; and has found that the assertion has no foundation in fact.

On Oats. By the Rev. Mr. CAMPBELL.—*Farmer's Mag. No.* 40.

THIS account gives the result of experiments made on oats in 1808. Two ridges were sown broadcast and harrowed in the usual manner; two others, after being harrowed, had the sides of the ridges pared by the plough; and the earth thus gathered into the furrows was, by means of a shovel, thrown over the seed sown upon the ridges. The ridges were three feet broad, and the crop was after potatoes, and was kept clean. The produce of the ridges covered by the shovel was greater than the produce of the ridges harrowed in the usual way, in the proportion of eight pecks per acre.

Observations.—This experiment does not appear to be otherwise important, than as shewing the accuracy of Mr. Campbell in his agricultural pursuits.

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On the Winter Management of Cattle. By ARATOR.—*Farmer's Magazine, No.* 40.

THE winter management of cattle is represented to be a subject of the first importance to the great body of the farmers, and it concerns not only the feeding off their turnips and other winter crops, but also influences in a considerable degree the quantity of manure obtained. Passing over the question whether it be most beneficial in point of profit from the cattle themselves to stall-feed them with turnips, &c. or to keep them in open yards, giving their food in troughs and cribs, it is contended that the latter mode is by far most conducive to the health of the animal, and consequently better calculated to furnish the public with more sound and wholesome beef than is supplied by the stall-feeders: but it is noticed that not more than six ought to be put into the same yard, and that these ought to be of equal sizes and tempers. It is also asserted that this method converts more straw into dung, than can be done by stall-feeding.

The importance of keeping store cattle in an improving state during their abode in the straw-yard in winter is very properly insisted on; and it is recommended to effect this by giving them a small quantity of turnips from the time they enter the straw-yard, and gradually increasing the allowance as the season advances, and as the straw loses its nutritive powers. By these means no time will be lost in the improvement of the cattle when they first go out to grass, as is frequently the case for a month, when they are suffered to eat nothing but straw in the yards; whereas, in the other mode, improvement takes place immediately, and they are much more speedily fattened. And with regard to the quantity and quality of the dung, the want of turnip feeding is stated to be of incalculable loss.

Observations.—Very sensible and useful remarks evidently conveyed by a practical farmer without any affectation of superior learning and information.

On human Exuviæ, and Soil-holes. By COPROPHILUS.—*Farmer's Magazine, No.* 40.

AMIDST much desultory writing which has little or no connection with the subject, Coprophilus recommends that all the ashes of the family be constantly thrown into the soil-hole, which would so completely neutralize its contents that no offensive smell would exist even in the closest and most confined yards. And it is stated

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that if this practice were pursued, and the seat made to open in front by a door, the hole might be emptied with much convenience at any time by opening the front board, and shovelling the contents into a tub or cart; for so completely would every thing be absorbed by the ashes, that no dirt would be made by the process, nor any offensive smell arise from it.

Observations.—We cannot too strongly recommend the advice of this writer for the purposes of both cleanliness and manure; but the effect of ashes to neutralize human exuvie is no new discovery; it has been known to the poor and indigent cottager from the earliest periods to which civilization can be traced.

Observations on the Causes, Prevention, and Cure of the Foot-rot in Sheep. By G. F. H.—*Farmer's Magazine, No.* 40.

Foot-rot is defined to be a suppuration of the glands between and above the hoofs of sheep; and is said to bear a very striking analogy to chilblains in the human species. The disease most commonly affects the fore feet, but sometimes extends to all; the seat of it is the external part of the hoof, and an acrid and fetid humour exudes from the cleft. It is represented that when this disease is not arrested in its progress, the inflammation round the hoof becomes black, and degenerates into an ulcer, which occasions such pain as to make the animal walk on its knees, and in some instances the hoofs drop off. It is remarked that the disease seldom appears on dry clean sheep-walks, but mostly on wet and dirty pasture.

The *cause* of this disorder is said to have been attributed to various circumstances; but this writer conceives it to be occasioned more immediately by the sheep being kept on a wet soil, and by feeding alternately on wet and dry ground in warm weather: with the exception of salt-marsh and peat-moss, where the disease is seldom discovered.

The *prevention* and *cure* are truly stated to be the primary objects of investigation. The recommendation of Mr. Lawrence, in his Treatise on Cattle, to apply oil of turpentine and brandy wherever fissures or cracks make their appearance in the foot, is admitted to produce a beneficial effect in general; but it is contended that where the disease has been long seated, recourse must be had to caustics, of which sulphuric acid and the nitrate of mercury are held to be the best; after the application of which, the foot should be bound up and kept dry till recovery. But as even this is not always successful in inveterate cases; a mode of treatment is laid down, which, it is asserted, may be depended upon as a certain cure; and which, for that reason, we give in the words of the writer: "Whenever the disease makes its ap-

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pearance, let the foot be carefully examined, and the diseased part well washed and pared as nigh as possible, not to make it bleed; and let the floor of the house where the sheep are confined be strewn three or four inches thick with quick lime hot from the kiln; and the sheep, after having their feet dressed in the manner above described, to stand in it during the space of six or seven hours."

Observations.—The *foot-rot* is treated in this paper in respect to its description, cause, and cure, with a scientific accuracy, to which so troublesome and injurious a disease is justly entitled. We have no doubt that the treatment recommended may be received as a specific remedy, and we have long experienced lime to be the most effectual antidote to its deleterious effects.

Plan for destroying Foxes, and for relieving Country Gentlemen from the great Trouble hitherto taken by them of freeing the Country of these Animals. By B.—*Farmer's Magazine, No.* 40.

The plan recommended is for farmers to meet periodically with vast numbers of greyhounds, and chase to death all the foxes they can meet with, and give a reward of not less than twenty shillings a head for every fox destroyed by poison, guns, or pitfalls, or in any way, except hunting. And it is contended that this might be done for a less expense than repairing the gates and fences broken down by hunters. The whole article is an unsuccessful attempt at irony.

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